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PART I.

ORIGINAL COMMUNICATIONS.

ART. XI.—*On Laceration of the Vagina in the Course of Labour.* By ALFRED H. M'CLINTOCK, M.D., F.R.C.S.; President of the Dublin Obstetrical Society; late Master of the Lying-in Hospital, &c., &c.[a]

EXPERIENCE abundantly proves that any part of the genital tract may be lacerated in the progress of labour. The *nature* of the accident is the same, wherever situated, but the causes, the symptoms, the prognosis, and the treatment will materially differ according to the particular situation in which the solution of continuity takes place.

The part most commonly torn is the perineum; next, in the order of frequency, is the lower third or half of the vagina posteriorly, involving, perhaps, the rectum; then the cervical portion of the uterus; next, the upper third, or peri-uterine portion of the vagina; and least frequently the fundus and superior region of the body of the uterus.

Ruptures of the perineum and lower part of the vagina are of common occurrence, easy of recognition, and nearly quite devoid of danger; at the same time I do believe—and I gave some attention to this very point—that these accidents predispose in some measure to uterine or abdominal inflammation, just as any operation

[a] Read before the Dublin Obstetrical Society.

in this same region may do with a healthy non-puerperal woman. Their comparative freedom from danger is, in a great degree, attributable to the exemption of the peritoneum from any participation in the laceration or its effects. I say "in a great degree," because rapidly fatal ruptures of the uterus do take place, in which the peritoneum is wholly free from implication; whence we may infer that the danger does not depend solely on the peritoneum being involved, though this adds materially to it.

Whilst the pathology, diagnosis, and treatment of ruptures of the *uterus* are fully described in all the modern English treatises upon midwifery, strange to say no separate or distinct consideration is given to lacerations of the *vagina;* these are just incidentally mentioned, or spoken of as though identical, or nearly identical, in all particulars save situation, with uterine ruptures. Indeed, with the exception of Goldson, whose pamphlet was published in 1787, English authors have taken little or no notice of vaginal laceration. Of continental obstetric authors, however, there are many—and I would mention particularly Boer, Baudelocque, Moreau, Lachapelle, and Danyau—who recognise many of the points of difference between these two lacerations of the genital canal. I cannot, therefore, lay any claim to originality in this memoir. My sole object is to bring before the society, in a brief but connected manner, the pathology, symptoms, and treatment of the accident, together with the history of some examples which fell under my own observation.

The vagina is an organ differing essentially from the uterus, in its structure, relations, functions, and pathology. Hence it is reasonable, *à priori*, to assume that the lesions it sustains in consequence of parturition would have some proper and distinctive characters. I fully admit there is a general resemblance in the symptoms between ruptures of the uterus and lacerations of the vagina, that both are highly dangerous, and that in both classes the primary indication of treatment is the same. Still there are good reasons why we should recognize the differences between them. Accuracy of diagnosis must precede all improvements in practice; and by establishing a clear distinction between these two accidents, and studying each by itself, we shall obtain a fuller and more correct knowledge of the diagnosis and the treatment of both these most formidable complications of childbed.

Lacerations of the vagina—the upper part I mean—are not so frequent a consequence of parturition as are ruptures of the uterus. Let me here make an observation. Collins, and many other

observers, when describing the seat of a laceration, employ the expression, "at the junction of the cervix and vagina," or some phrase of like import. Now, all such cases—their number is but very small—I include under the heading of vaginal laceration, not merely because their claims to being considered uterine or vaginal are equally balanced, but because in some important particulars they more resemble lacerations of the vagina than of the fundus or body of the womb.

To determine the comparative frequency of lacerations of the vagina would be interesting and important; but, unfortunately, most of the published statistics of lacerations of the genital canal are not sufficiently exact in recognizing the site of the injury to furnish data for settling this question. The most extensive collection on record of such cases is that contained in the valuable monograph by Dr. Trask; but, though purporting to be "On Rupture of the *Uterus*," it includes, without distinction, cases of laceration of the vagina. I have no doubt whatever that if practitioners and authors were always careful to ascertain and define the exact seat of laceration, we should find the accident in question to be far more frequent than anyone supposes. Moreau, indeed, distinctly says he could adduce, from his own experience, as well as from the works of authors, very many facts proving that, in the larger proportion of cases, rupture of the vagina has been mistaken for rupture of the uterus.

But, laying aside conjecture, let us see what data we possess to decide this important question. Dr. Collins records 34 cases of ruptures of the uterus and of the vagina; in *eleven* of these the rupture almost exclusively engaged the vagina. Drs. Johnston and Sinclair, in like manner, describe 17 cases, of which *six* were instances of laceration of the vagina only. In my own hospital experience I met with 11 cases of ruptured uterus and *five* cases of rupture of the vagina. Now it is worthy of remark that in the experience of Dr. Collins, Dr. Shekleton (as recorded by Drs. Johnston and Sinclair), and myself, respectively, the comparative frequency of laceration of the vagina is nearly the same. For example, Dr. Collins had 11 cases out of a total of 34, or one-third; Dr. Shekleton had 6 out of 17, a fraction over a third; Dr. M'Clintock 5 out of 16, or a fraction below one-third. To these we may add the cases of Dr. M'Keever, who met with three examples of lacerated vagina and *ten* of rupture of the uterus, or uterus and vagina. Dr. Joseph Clarke, in his report of the practice of the hospital, relates 8 cases

of rupture, *seven* of which were examples of lacerated vagina; and so, most probably, was the other case, which ended in recovery; but as this might be disputed I shall not reckon it. During Dr. Charles Johnson's mastership of the hospital (1840 to 1847) there occurred 20 cases of ruptures of the uterus and of the vagina, and amongst these were *three* instances of undoubted laceration of the vagina at its junction with the os uteri. To sum up, then, we have here a total of 108 cases of uterine and vaginal lacerations—all occurring in the progress of labour—and out of this number we find *thirty-five*, or over one-third, to be ruptures confined to the vagina, or to the vagina and os uteri.

It would hardly be assuming too much if, from this collection of cases, we were to deduce the average frequency of vaginal ruptures as compared with uterine. I know very well that a much larger proportion than three-fourths of all the recorded cases of ruptures are represented to have been of the uterus; but it must be remembered that the older writers were not very precise in their anatomical descriptions; and, moreover, the names which were applied to the different portions of the genital passages were too vague to insure clearness or exactitude. For example, the term "collum uteri," which every tyro would now render neck of the womb, was used by Bartholinus, Columbus, and other anatomists, to designate the vagina.

Danyau, writing about the year 1850, was able to collect 17 cases of ruptured vagina, in which the fetus escaped into the abdominal cavity. As this result does not take place oftener than once in every four or five instances, it is plain that the total number of cases of ruptured vagina to be found scattered through the pages of obstetric literature must be pretty considerable. But it would be a very serious error to suppose that all these cases, if collected, would show the actual rate of mortality arising from this accident, or the relative frequency of vaginal rupture as compared with uterine. These are questions which can only be deduced from extensive and complete series of cases, where none are withheld; and the statistics I have given, from the records of the Rotunda Lying-in Hospital, fully answer this requirement, and, therefore, so far as they go, constitute safe and proper grounds to draw conclusions from.

If we look to the relative thickness of the uterus and of the vagina at the period of labour, it does at first seem a little strange that the former should so much more frequently be the seat of

aceration. The wall of the vagina when much distended, as it is by the fetus, is really very little thicker than chamois leather. I was greatly struck by this in a recent case where I had to remove an incarcerated placenta. The os uteri offered considerable resistance to the passage of the hand, the vagina, in consequence, being put very much on the stretch; and so distinctly did I feel the sacrum and the bodies of the lumbar vertebræ through the distended vagina, that for a moment I scarcely conceived it possible my hand was not fairly in the cavity of the abdomen.

Although, however, the vagina be considerably thinner than the uterus, yet it is comparatively stronger, by virtue of its dense fibro-elastic external coat, to which the uterus possesses nothing analogous in point of structure. It is highly probable, too, that in many cases, especially amongst pluriparæ, the uterus, when occupied by the fetus, and its fibres in a state of relaxation or extension, actually possesses very little more of thickness than does the vagina. Meigs supposes that in most cases of rupture the tear commences in the posterior wall of the vagina, near to the cervix, from its remarkable thinness in this situation. But however probable this might appear from *à priori* reasoning, still it is not at all in accordance with the results of experience, as in a very large majority of the cases of utero-vaginal laceration—not to mention the cases of purely uterine laceration—the rent is situated at the side, and not behind.

Clinical observation teaches us that any part of the vaginal canal may be torn except the lower portion of the anterior wall, which part, though the most common seat of sloughing, was exempt from laceration in all the histories I have perused. The nearest approach to a rent in this situation was a case recorded by Drs. Johnston and Sinclair, in which there was a transverse slit in the base of the bladder, just at its relation to the vagina, and apparently not engaging the peritoneum. In the upper peri-uterine portion of the canal rupture occurs, in front and behind, with pretty nearly equal frequency. When situated anteriorly the bladder is sometimes implicated in the laceration, of which the case just alluded to is an example. The same also occurred, though to a far greater extent, in Goldson's, and one of Dr. M'Keever's cases.

In a considerable number of cases the laceration is situated somewhat more to one side or the other. But wherever it may be it almost invariably takes a circular direction, often extending through one-half, and sometimes three-fourths, the circumference of

the canal. On a few rare occasions the vagina has been almost entirely detached from the uterus. In Dr. M'Keever's twelfth case this was well seen. "The body of the uterus," he says, "was nearly altogether separated from the vagina, being merely retained by a slender thread on the left side." The tear once begun is easily enlarged, either by the continuance of the pains or by incautious attempts at artificial delivery with the hand or with instruments. Of the cases where the rent was chiefly at the side of the vagina the left was more frequently engaged than the right. It is just possible that the laceration may take a longitudinal direction in the posterior wall. Again, it may have the appearance of a somewhat round aperture, as in one of Collins' cases. It is deserving of notice that in nearly all cases where the laceration was of spontaneous origin—*i.e.*, not the result of manual or instrumental interference—it has taken more of a circular than longitudinal direction. In uterine lacerations, on the contrary, the prevailing direction of the laceration—except, perhaps, where the os alone is engaged—is more or less longitudinal.

Whether situated in front or behind, in nearly every instance the laceration has extended through the vagina and into the peritoneal cavity. Dr. Churchill alludes to a case of anterior laceration of the vagina, in which he supposes, and with much probability, that the serous coat did not give way till just before her death on the fifth day. In one of Dr. Collins' cases there existed a rent in the same situation, the peritoneum over it being entire. Another case is related by him of extensive utero-vaginal laceration posteriorly, and the serous membrane covering the rupture was intact.

The following may be mentioned as the most influential predisposing causes of vaginal laceration, viz.:—

1. Disease of the vagina.
2. Disproportion between the size of the child and the capacity of the pelvis.
3. Osseous irregularity on the internal surface of the pelvis.

1. The diseased condition of the vagina which has most commonly led to its rupture was narrowing, or contraction with cicatrices, the result of former inflammation and sloughing. Both Dr. Collins and Drs. Johnston and Sinclair give examples of laceration arising out of this vicious state of the vagina.

2. Decided contraction of the pelvis was observed in only a very small proportion of cases, whilst in a smaller number the disproportion was due to a hydrocephalic condition of the fetus.

3. Denman was the first to suggest that the attrition sustained by the uterus (or vagina), between the fetal head and an abnormally salient promontary of the sacrum, might cause it to lacerate. Dr. Collins narrates a case where the solution of continuity in the vagina corresponded very accurately with the unusually projecting promontary of the sacrum; and Drs. Johnston and Sinclair, when describing the *post mortem* appearances in their eighth case, observe:—"A laceration of the vagina was found to exist, leading from a point anteriorly towards the left, and about four inches in extent. The pelvis showed no diminution, but on the internal surface of its anterior wall a prominent ridge was observed, about an inch long, and running in a direction from the centre of the junction of the pubic bones downwards and to the left."

In every instance that I know of, where vaginal laceration was spontaneously produced, the head was the presenting part. A case is recorded by Dr. Collins which forms a partial exception to this statement. Here the breech presented and the delivery was easily effected; but the uterus was extensively torn along with the vagina, so that I do not reckon this case amongst those of vaginal laceration. In not a few cases there was presentation of the upper extremity, and it was in the efforts to rectify this malposition of the fetus that the practitioner was so unfortunate as to lacerate the vagina.

The number of the labour does not seem to have much influence upon the production of the accident, beyond this, that it is proportionately much less frequent in first than in second, third, or fourth labours respectively. Of forty-eight cases where this circumstance was noted, I find *six* were primiparæ and *forty-two* pluriparæ, viz.:—*Ten* in the second labour, *eight* in the third, *nine* in the fourth, *two* in the fifth, *five* in the sixth, *three* in the seventh, *two* in the eighth, and *one* in the ninth, tenth, and eleventh labours respectively. As regards the sex of the child the same law holds good as in ruptures of the uterus, the majority being males; out of twenty-seven cases *fifteen* of the children were boys and *twelve* were girls.

In regard to the mode of its production it is easy to comprehend that inordinate uterine action must play the most important part; but powerful uterine contractions alone will hardly cause laceration of a healthy vagina with a normal pelvis, unless there be some unnatural roughness or projection on the interior of the pelvis. But if there be any obstacle or hindrance to the advance of the fetus, after its head has cleared the os uteri, under these circumstances,

the vagina will have to sustain a degree of traction or extension exactly equal in amount to the strength of the pains. Hence it is often found in practice that for the production of the laceration, uterine action and obstruction from some cause or other have concurred.

In almost all cases the head had not only entered the pelvis, but in the majority of them was fully engaged in its cavity at the time of the rupture occurring; and the plain inference to be drawn, I think, from these facts is, that contraction of the brim of the pelvis is not by any means so influential a cause of laceration of the *vagina* as it is of rupture of the uterus, especially of its neck. Simple over-distension of the vagina, as from the forcible extraction of an emphysematous child or hydrocephalic head, might cause it to burst. In this case the greater the capacity of the pelvis the more risk would there be of rupture. On very many occasions the vagina has been torn by attempts to force the hand into the uterus for the purpose of turning the child, or of rectifying some real or fancied malposition of the head. It has also been lacerated by the premature or unskilful use of the forceps.

Many other causes for rupture of the vagina are laid down by authors, such as violent movements of the fetus, obliquity of the uterus, attitudes of the mother, &c., &c., which I do not think it worth while to stop and examine, as they seem to be drawn more from theoretical consideration than clinical observation. That mechanical violence, inflicted on the abdomen during labour, might be competent to tear the vagina not only seems highly probable but is confirmed by a case related in the work of Drs. Sinclair and Johnston.

With respect to the etiology of rupture there is one point on which the vagina and uterus stand in marked contrast, namely, that whilst the latter is frequently burst by its own active contractions, the vagina never is, nor can be.

Though I have thus described many causes for the production of this accident, it must still be confessed that in no small number of cases there was really no assignable cause for its occurrence—no pelvic deformity, no malposition, no disproportion, no violent uterine action—so that we have yet much to learn upon this point in the history of the lesion. Some of the older writers regard vaginal laceration as a more fatal accident than uterine laceration. This opinion was not deduced from any clinical facts, but seems to have been based on the observation that a rent in the uterus underwent

a diminution of size in consequence of the contractions of the organ; whereas the absence of any such contraction in the vagina allowed the laceration of this canal to retain its full size. The reasoning seems plausible, and the facts on which it rests are undoubted; nevertheless it is a fallacy, and supplies an apt illustration of the danger of *à priori* conclusions in medicine.

The *prognosis* to be formed of laceration of the peri-uterine portion of the vagina must always be grave, but yet qualified according to the circumstances of the particular case. Let us first inquire what is the average mortality in the general run of cases of this accident, and then examine what are the special conditions which should guide us in forming a judgment upon any individual case. I am sorry to say we do not possess any adequate trustworthy body of facts from which a conclusion might be drawn respecting the former of these questions, *i.e.*, the average mortality among cases of this accident. Dr. Trask's statistics we can not safely use for this purpose in consequence of his observing no distinction between cases of rupture of the vagina and rupture of the uterus. A collection, even, of all the published cases of ruptured vagina, valuable though such would be, would not supply just data to go upon, and for the simple and obvious reason, that they do not represent *all the cases* that have actually occurred, but consist chiefly of the exceptional ones—picked cases—those, namely, which were remarkable from the fact of the patient recovering.

To form any, or the barest, approximation to the truth upon this important point, therefore, I am obliged to go upon the data supplied by the experience of the Rotunda Lying-in Hospital, though fully aware that these data are not by any means sufficiently extensive to yield the true average.

This experience, then, furnishes thirty-five cases of laceration of the vagina, and of these four patients recovered; whilst of the seventy-three remaining cases, where the rupture was exclusively or chiefly confined to the uterus, only *three* recovered, *one* under Dr. Clarke, *one* under Dr. M'Keever, and *one* under Dr. M'Clintock; but it is very questionable whether the first and last of these cases should not be included in the former category, namely, lacerations of the vagina. Leaving them as they are, however, there is a wide difference between four recoveries out of thirty-five cases and three out of seventy-three—in other words, between twelve per cent. and four and a quarter per cent.

It would be highly interesting and important to know the actual

rate of mortality among cases of rupture of the uterus; but in truth we do not at present possess the requisite data for determining this point. Dr. Trask's most valuable and elaborate statistics cannot, for reasons already mentioned, yield a correct solution of the question.

My own researches lead me to believe that rupture of the uterus above the os is far more dangerous than laceration of the vagina, and that a large proportion of the cases published as examples of recovery from rupture of the uterus were in reality examples of laceration of the vagina, or of the vagina and os uteri. This idea is not original; Professor Boer of Vienna, Professor Dubois and M. Danyau of Paris have expressed a similar opinion. Danyau collected seventeen cases of ruptured vagina, in which the fetus escaped into the cavity of the belly, and of these cases *four* ended in recovery. I have collected, from easily accessible sources, fifty-one cases of laceration of the vagina, including *thirteen* instances of perfect recovery.

From the foregoing facts and observations I think it is justifiable to draw the conclusion that the danger to life from vaginal laceration is very much less than from uterine laceration. A most interesting question here presents itself. What is the cause of this difference? Why should rupture of the uterus be so much more dangerous than that of the vagina? In very many fatal cases (Trask mentions twelve) of uterine laceration the peritoneum is unbroken; and of the *thirteen* recoveries from vaginal laceration, in my collection, *twelve* had the peritoneum involved in the tear. We may safely assume, therefore, that the implication or non-implication of the serous membrane has nothing whatever to say to this difference. Mr. Goldson, who has the merit of being the first English writer to draw attention to the subject of vaginal lacerations, assigns as a reason of their being less dangerous than uterine lacerations that the delivery of the child is accomplished with so little difficulty. This refers, I presume, to the instances where the child has escaped into the cavity of the belly; of such cases, and of them only, the assertion is strictly correct.

If we look to the comparative importance of the two organs, as indicated by their organization and by the effects of their diseases, especially puerperal diseases, we must at once admit that the uterus stands much higher in the scale than the other.

Again, the changes which are set up in the uterus, immediately on the completion of the act of parturition, are most unfavourable

to the repair of any injury or wound of its structure. A general disintegration and removal of the uterine structure is going forward, attended by a degradation of its tissue. This series of processes has been well described by Dr. C. West, and is looked upon by him as an influential cause of the great mortality following the Cæsarean operation.—*Med. Chir. Trans.*, Vol. XXXIV.

In forming a prognosis upon any individual case the exemption or implication of the peritoneum is a circumstance which, if known to us, should have considerable weight. The cases in which it escapes are extremely rare; and when the tear engages the posterior region of the peri-uterine vagina it is hard to conceive it possible for the serous cavity to avoid being opened. I only know of three cases where the peritoneum was stated not to be involved, and one of these recovered. Dr. Collins gives a case where the rent was situated anteriorly, not involving the peritoneum, and yet the woman died almost immediately upon delivery. We have already seen that in several of the recorded fatal cases of uterine rupture the serous covering of the womb was intact. All these facts would seem to show that whether the peritoneum be engaged or not has comparatively little influence upon the result.

Another important circumstance to take into our consideration, in reference to prognosis, is the amount of hemorrhage. A great source of danger in all these cases is the extravasation of blood into the peritoneal cavity. The experience derived from operations upon the abdomen gives the strongest confirmation to this.[a] We cannot always know, however, whether any or much blood be effused internally, as the quantity of external discharge is not an infallible index, though supplying ground for probability

Of course the greater the extent of the laceration the greater, *cæteris paribus*, will be the danger.

The severity of the constitutional symptoms immediately supervening on the accident deserves careful consideration in estimating the chances of recovery. A great amount of shock would indicate either an extensive injury, or large effusion, or a highly irritable

[a] Some surgeons are of opinion that effused blood has not, by itself, an irritating effect upon the serous membrane, but that it is the presence of air along with the blood that does the mischief, by inducing decomposition of the latter. Decisive facts are wanting to clear up this point, as the circumstances under which blood most commonly enters the serous sac will also allow the admission of air. Even should the opinion of these surgeons be confirmed, the statement in the text will not thereby be affected, as in nearly every instance of vaginal laceration there is almost nothing to prevent the admission of air.

state of constitution, rendering the patient a bad subject for any accident or operation. The almost total absence of shock in the two cases of recovery from extensive vaginal laceration recorded by Dr. Ross, of Hamburg, was a very remarkable feature. These two cases occurred in the same woman, in her fourth and fifth labours.

Whether the rent be situated anteriorly or posteriorly does not seem to have any influence upon the result.

The length of time elapsing between the occurrence of the accident and the delivery of the patient is an important element in our calculation. The shorter this interval the better for the patient.

The escape of the child through the rent into the cavity of the belly must be regarded as an aggravating circumstance; and yet Danyau's collection of cases, already alluded to, in all of which it occurred, show good results, namely, four recoveries out of seventeen cases.

I need hardly add that the presence of an unreduced portion of intestine in the vagina would almost deprive the patient of any chance of recovery. In one such case (recorded by M. Percy, and mentioned in Baudelocque) the patient died the third day, with all the symptoms of strangulated hernia. On the other hand, to show us we should never despair, there is the memorable case of Dr. M'Keever's, in which nearly four feet of intestine prolapsed through the rent in the vagina, and eventually sloughed off; and yet this patient recovered.

The train of symptoms, when fully developed, which vaginal laceration produces is nearly the same as that following upon uterine rupture. There is no symptom peculiar to the one or to the other accident, so far as we at present know, and, therefore, no single symptom pathognomonic of either. Nevertheless, as regards the frequency and the prominence of individual symptoms, experience does show a considerable difference between the two lesions.

Before going further I should remark that there are no premonitory symptoms, properly so called, of laceration of the vagina. Of course we might apprehend its occurrence in a patient whom we know to have a diseased vagina, or a narrow outlet, or any osseous projection of the pelvis; and, in any case, the continuance of powerful expulsive pains, without advance of the head, after this had entered the pelvic cavity, should suggest to our mind the possibility of the vagina giving way. If one or other of the above

predisposing circumstances were present in the case the possibility would be changed into a strong probability.

Let us now briefly review the *symptoms* which may follow upon the accident in question. The number and severity of these will, as a general rule, bear a proportion to the extent of the injury and to the constitutional irritability of the patient. The latter has, I believe, the more influence, a great deal, of the two, in the production of any shock as a consequence of the lesion. It is reasonable to suppose that the exemption of the peritoneum from implication in the laceration would tend to lessen or avert the disposition to nervous shock. Certainly in two out of three cases where the peritoneum was not involved there was a total absence of symptoms of collapse Even where the serous membrane is extensively torn there may be comparatively no indication of shock to the system, as in the following case of my own:—

Case I.—A short plethoric woman was admitted, in labour of her first child, in the afternoon of 25th December. The dilatation of the os uteri was not completed for nearly thirty hours; but the second stage was short, and a dead female child was born, by the natural efforts, at 5 a.m. of the 27th. When seen a few hours afterwards, at morning visit, her pulse was observed to be very frequent, but there was no other symptom to arrest attention. At 7 p.m. of same day the pulse had risen to 126, and was extremely weak; the face was rather congested; the tongue dry and coated; the belly full and tympanitic, and extremely tender to pressure; the respiration laboured. The woman was evidently in the most imminent danger. I should remark that she had no vomiting then, or at any time previously since the commencement of her labour. She expired at 8 a.m. of the 28th. There was a good deal of bloody effusion into the abdomen, with marks of incipient peritonitis. In the posterior part of the vagina, just below the os uteri, was an extensive laceration involving all the tissues of the canal, and engaging nearly three-fourths of its circumference. The perineum was torn, and the vulva was rather sloughy.

On the other hand, with no greater amount of injury than occurred in the case just related, the effect on the vital powers may be so overwhelming as speedily to extinguish life, of which the following case (which occurred when I was a pupil at the Lying-in Hospital, in Dr. Charles Johnson's mastership) is a good example:—

Case II. occurred at the hospital in the year 1841. The woman was in her fourth labour, and before admission had been eighteen hours ill. The arm and shoulder were presenting; turning was effected without any unusual difficulty, and a living boy extracted. The patient at once began to sink, and expired in two hours, despite of everything that was done to sustain and rally the vital powers.

At the *post mortem* examination an extensive laceration was discovered in the posterior part of the vagina, and just at its junction with the os uteri.

Doubtless the operation of version here contributed somewhat to the shock under which the patient sank.

Setting aside exceptional cases, then, I think it will hold good as a rule that the symptoms of collapse are not developed so soon, or in such a marked manner, after vaginal laceration as they are after uterine rupture. I only speak of what occurs at the outset. Of course, with the lapse of time, and if the woman be not delivered, it is to be expected that marked indications of depression of the vital powers will present themselves. Thus, in the following case there was an interval of nearly seven hours from the occurrence of the laceration until the woman was brought into the hospital, which is sufficient to account for the symptoms of prostration being so decided at the time of my seeing her:—

Case III.—This woman fell in labour of her fourth child on the 22nd October, 1859. Early in the morning of the 23rd she was seen by a pupil, who, finding her case dangerous, and her circumstances the most wretched that can well be conceived, brought her off to the hospital in a car. I first saw her at 8 a.m.; she was then in a state bordering on collapse, with a scarcely perceptible pulse, cold surface, pinched face, some vomiting, and slight red discharge from the vagina. She complained of great pain in the lower belly, with extreme tenderness on pressure; and she said that the labour pains had suddenly ceased between 1 and 2 o'clock, a.m., since when the soreness of the belly had come on. The fetal heart was nowhere audible; the head had partially descended into the pelvis, and lay transversely, midway, in fact, between the third and second positions. The proper uterine tumour, hard and defined, could be felt at the level of the umbilicus; above and behind it a movable tumour could just be distinguished, which I took for granted was part of the fetus; but I could not minutely examine it owing to

the extreme tenderness of the abdomen. There had been some vomiting before her admission to the hospital. I delivered by perforation; the child, a girl, was evidently dead for some hours. The placenta came away immediately, with slight traction of the cord. She was very weak after delivery, but rallied in the course of a few hours, under the diligent use of restoratives. Vomiting and debility were the prominent symptoms during the four days that she survived.

Autopsy.—Extensive peritonitis, with large quantities of lymph; uterus pretty well contracted; the vagina separated from its uterine attachment, posteriorly, by a large transverse rent about four inches long; the conjugate diameter of the brim measured three inches and three-quarters.

I have no doubt whatever it was the fetal buttock or lower extremities that we felt before delivery, above and behind the uterus, as the same has been noted by other observers in cases where, as here, the head was still in the pelvis. Bearing in mind the position of the rent, I confess I cannot clearly comprehend how the trunk of the child gets out of the uterus into the abdomen, its head remaining all the while in the pelvis. To pass through the rent head-foremost would be intelligible enough; but in the cases I speak of the head does not change its position, or is the last part to recede out of the genital canal.

By some it is supposed that the severity of the constitutional shock depends on the amount of hemorrhage resulting from the laceration. Though I would not altogether exclude hemorrhage from having some effect this way, yet hemorrhage in any considerable quantity is rarely present; and, besides, both analogy and experience justify us in considering that its influence must be comparatively very small.

Vomiting is by no means constantly present in vaginal laceration, and the matter ejected very rarely possesses the coffee-grounds character. This latter character was wanting in each of my cases. Nevertheless vomiting is a symptom whose appearance in the second stage of labour should always put the attendant on his guard, and awaken a suspicion of rupture somewhere.

The same may be said of *hemorrhage* coming on after the woman has entered the second stage of her labour. When the consequence of rupture the sanguineous discharge is seldom profuse, or sufficient to occasion, by its quantity merely, any alarm for the safety of the

patient. It is a symptom pretty generally present in ruptures, and more so, perhaps, in vaginal than in uterine ones. The contrary to this might have been expected from the greater size and number of the uterine vessels. But the vagina is surrounded by a venous network, capable of yielding blood for a considerable time, in consequence of the entire absence of valves, and of any special arrangement (such as the uterus possesses) for compression of the bleeding vessels.

More or less complete *cessation of the labour pains* occurs in most instances of lacerated vagina. In a few, however, the uterus continues to act, assisting the removal of the fetus, or even effecting its expulsion as well as that of the placenta. This actually took place in four out of the fifty-one cases which I have tabulated. One of these occurred under my own observation, and was the first case I related; another occurred to Dr. Collins; a third is recorded by Dr. Ingleby, and a fourth happened in the practice of Smellie. The very fact of the escape of the fetus through the laceration—an occurrence very far from uncommon—supposes the presence of uterine contractions of moderate strength, at all events.

Case IV.—This was a large stout woman, in her second labour. The os uteri was fully dilated, and the membranes ruptured, about midnight, after which time the uterine contractions became very strong and frequent, and she herself was restless and unmanageable. When I saw her, at 9.30 a.m., the pulse was 112; there was a large tumour on the head, which had descended into the pelvis, but not entirely, and was firmly wedged there; the pains were apparently most powerful, but making no impression whatever on the fetal head; she was very restless, and the sides of the belly were tympanitic. The most careful auscultation failed to detect any trace of the fetal heart.

I felt that she should be delivered without delay, though I freely confess I did not suspect the existence of any laceration.

After perforation the head was brought away without difficulty; the child, a boy. On dividing the cord the blood contained in it was found to be coagulated. The pulse continued frequent from the time of delivery, and the same evening she complained of pain in the belly, which was full and tender. At seven o'clock of the next morning, after a restless night, she suddenly began to sink, and expired within an hour.

Autopsy.—Well-marked peritonitis, with lymphy exudation;

serous and sanguineous effusion in the lower belly; in the posterior and left side of the vagina, rather low down, and freely communicating with the peritoneal cavity, was an extensive laceration. The pelvis appeared of normal size.

This case was remarkable, first, for the absence of all the characteristic symptoms of laceration; and, second, for the sudden and unexpected manner of her death.

I have related the case chiefly to show that the pains may go on after the occurrence of a vaginal laceration; but it also illustrates the difficulties which occasionally beset the diagnosis of the lesion in question. In this respect the case bears a strong resemblance to the first one detailed.

Contrary to the opinion of Churchill and some other authors, *recession* of the presenting part does take place, slightly, in the greater number of cases of vaginal laceration, and totally in those cases where the fetus slips into the sac of the peritoneum. This last symptom (the escape of fetus into belly) is much less rare in vaginal than in uterine lacerations. For this two reasons may be assigned—first, the continuance of uterine contractions, and second, the uncontracted patulous state of the rent in the vagina. The same reasons will serve to explain why the placenta on some occasions follows the child into the abdominal cavity.

Where the fetus has partially or entirely escaped out of the uterus and into the belly, its limbs and body are readily distinguishable through the abdominal parietes, unless these be enormously loaded with fat, or the belly become very tender. Sometimes the contracted uterus may, by the same mode of examination, be felt and recognized with great ease. Short of feeling the laceration itself, there is no more demonstrative proof of its existence than the symptoms just mentioned. But it is only within a limited, though variable, period that the child can be thus distinctly recognized. In the course of a few hours the belly becomes swollen, and tense, and tender, and the patient connot tolerate any manipulation of the part. Under these circumstances it would be very difficult, or impossible, to discern the limbs of the extra-uterine fetus.

Prolapse through the laceration, of intestine or omentum, into the vagina, or even its protrusion externally beyond the vulva, is not an unfrequent complication of lacerations of this canal, though an extremely rare consequence of ruptures of the fundus or body of the uterus. This may, in great measure, be accounted for by the

uncontracted state and thin edges of the vaginal laceration. Of the fifty-one cases I have collected of vaginal laceration, there was a hernia of the intestine or omentum in *eleven*. This hernia in *two* instances actually occurred before the removal of the fetus; and in *one* most remarkable case (that recorded by Dr. M'Keever) it took place on the fourth day after delivery.

The position of the intestines above and behind the gravid uterus prepares us for what experience shows to be the fact, namely, that the prolapse of intestine is more apt to take place when the laceration is situated in the posterior wall of the vagina, though it has occurred when the rent was in front.

Cessation of the fetal heart's sounds—a useful corroborative sign of rupture of the uterus, and one to which I particularly drew attention many years ago[a]—would seem to be of equal value in ruptures of the vagina; for, of twenty children delivered naturally or by non-destructive operations, after laceration of the vagina, only one was born living. But this instance proves nothing, as the child was delivered by the very act—turning—which tore the vagina; so that the interval between the two events, viz., the laceration and delivery, could have been only a few moments. I think it very probable, however, from the lesser intensity of the symptoms in laceration of the vagina, that the death of the fetus does not supervene quite so soon as after rupture of the uterus. Unfortunately facts are still wanting to enable us to determine this important and interesting point.

Subcutaneous emphysema of the lower belly is a rare, but, when present, a reliable symptom of ruptured uterus.[b] I cannot say whether it ever takes place after vaginal laceration. I have not myself met with it, nor seen any mention of it in published cases; and I am disposed to think, for anatomical reasons, that it is much less likely to follow vaginal than uterine ruptures.

We now come to speak of the *treatment* of laceration of the

[a] See a "Memoir on the Use of Auscultation in the Treatment of Labours," published in Vol. iv. of this Journal (August, 1847).

[b] The first time, I believe, that emphysema of the hypogastrium was observed as a symptom of rupture of the uterus was in a case which fell under my care at the Lying-in Hospital, in the month of August, 1855, and of which an account was published in the number of this Journal for November, 1857, page 450. Since then the presence of this symptom has been observed by others. Dr. H. G. Croly, of this city, has published a remarkable case of laceration of the bladder, from fracture of the pubis, in which, reasoning from analogy, he was led by the presence of this symptom, and this only, to diagnose the fatal injury to the viscus. The accuracy of the diagnosis was verified at the *autopsy*.—*Vide* Medical Press for March 9, 1859.

vagina, and I venture to affirm that if there be a point of practice on which medical men are unanimously agreed it is this, that immediate delivery should be resorted to once it is ascertained, beyond a doubt, that the vagina or the uterus has been lacerated in the progress of labour. This may be laid down, then, as an incontrovertible axiom. The only conceivable exception to it is where the patient happens to be so prostrated as to seem unable to bear the shock of immediate delivery; here it might be necessary to delay the performance of the operation for a short while, till she was somewhat recruited by the administration of stimulants.

There are four ways by which delivery has been effected in the class of cases before us, namely, the forceps, the crotchet, version, and gastrotomy. Upon each of these I shall offer a few, and only a few, remarks.

If the child have escaped into the abdomen, or if the head have receded out of the pelvis, the use of the forceps is impracticable. Even in the more numerous class of cases where the head remains still in the pelvis, the forceps is rarely the most judicious mode of delivery, for this reason, that there often exists some disproportion between the head and the pelvis; and also because, as we have already seen, the fetus rarely, if ever, survives the laceration for many minutes. Where proof is afforded of the child's actually being dead, the forceps should never, in this or any other class of cases, be preferred to the crotchet as a means of delivery, inasmuch as the latter (crotchet) is the safer mode for the mother.[a] Among my cases of laceration of the vagina was one delivered by the forceps, not on account of the laceration, whose existence, indeed, was not suspected at the time, but for convulsions:—

[a] I am anxious to guard against the above observation being misunderstood, or its application carried too far. What I contend for is, that delivery by the crotchet is, *cæteris paribus*, a safer operation for the mother than delivery by the forceps; and I am fully convinced that reason, experience, and statistics rightly used, all concur in establishing this position. It is of great practical importance to have this question clearly determined, in order to guide us as to the mode of delivery in all those cases—and they form a numerous group—in which the fetus is dead.

In thus claiming for this operation almost the only merit which belongs to it, let it not be supposed that I am undervaluing the forceps. Quite the reverse. Craniotomy should never be an operation of *election* if the fetus be living. Any evidence of the child's vitality introduces into our calculations a new element which forbids all comparison between the crotchet and the forceps, and renders the former wholly inadmissible, except as a last alternative, and when no other resource is left us of saving the life of the patient. Here it truly is an operation of *necessity*.

Case V.—This was a large robust woman, in labour of her first child, and admitted at 4 p.m. At 7.30 the head was descending in the pelvis and the fetal heart was audible, but there was occasional vomiting. At 10 p.m. a severe convulsive fit occurred, whereupon she was immediately chloroformed, and delivered by the forceps of a dead male child. I was not present at the delivery, but I believe it was effected with ease. There was some post-partum hemorrhage, and at eleven o'clock she had a second fit. The next morning she was heavy and stupid, with a slender weak pulse, and frequent sickness of stomach. The existence of a small thrombus was discovered in the left nympha. At 5 p.m. of this day she had a third fit. She regained intelligence, but died on the fifth day, with obscure symptoms of metro-peritonitis. Curious to say, her pulse was never above 110, and the day she died it was only 88. She could not be persuaded to take the stimulants which were ordered for her.

The *autopsy* revealed peritonitis, and a sloughy condition of the interior of the uterus, which contained a portion of the membranes in a fetid state. An irregular aperture was found in the vagina, very low down, towards the right side, and rather behind. This rent did not communicate with the peritoneum

It may be supposed that the laceration here was caused by the forceps. I do not think so, however, and for the following reasons:—

1. The patient had vomiting in the second stage of labour.
2. The child was quite dead at birth, though ascertained to be living two and a-half hours previously.
3. The laceration was not situated in the place most likely to be torn by the unskilful use of the forceps.
4. The operator was not a novice, but one accustomed to the use of the instrument.

This is the only case I have met with where the peritoneum did not participate in the laceration. Of the two very grave accidents which complicated her labour, namely, convulsions and lacerated vagina, the former had the greater share, I think, in causing her death. In one of Collins' cases, also, the same double complication occurred in a primipara, and with a like result.

Whilst speaking on the use of the forceps I must beg leave to give the particulars of another case, as it helps to illustrate this part of my subject.

Case VI.—A woman was admitted at midnight, in labour of her eighth child. At 6 a.m. the os uteri was fully dilated and the pains regular. About seven o'clock there was a cessation of the labour pains, and a short time afterwards she vomited. As she did not make any complaint these symptoms attracted no particular attention, till she was seen at nine o'clock by Dr. Jennings, then senior assistant, who at once suspected that some laceration had taken place. Her pulse was now rapid and weak; there was vomiting, and some discharge of blood from the vagina; the fetal heart was wholly inaudible; there were no labour pains present, and the limbs of the child could be felt in the abdomen; the head was in the pelvic cavity. Being myself from home, Dr. Shekleton saw this patient, and at once proceeded to deliver her with the forceps, but in the attempt to do so the head receded quite out of reach; whereupon he introduced his hand, and extracted the child by turning: the placenta soon followed, and there was no hemorrhage. She was very slow in rallying, and was greatly annoyed with vomiting and meteorismus during the next three days. On the fourth day the belly was very much distended, and an enema was given in the hope of lessening this tympanitis. The bowels were acted upon by the enema, and at the same time a large quantity of grumous blood was discharged from the vagina. She died on the fifth day.

At the *autopsy* we found peritonitis, and much extravasated blood in the belly; the uterus was well contracted; there was an extensive laceration in the upper and posterior part of the vagina; this was rather to the left side, and took a somewhat oblique direction, so as to engage the os uteri in a slight degree; the pelvis was well formed, and there was no trace of any pre-existing disease of the vagina

The crotchet has, mechanically speaking, a wider range of applicability than the forceps; and, for reasons already assigned, is more frequently to be preferred in the cases under consideration. The introduction of the perforator requires some caution, else the head may be pushed out of the pelvis, which would probably necessitate delivery by turning. To avoid this we should, if possible, select a fontanelle where but little force would be required to penetrate, or fix the point of the instrument on a spot of the head as near to the side of the pelvis as possible, so that the opposite side may form a point of resistance. A slight curve in the extremity

of the perforator will here be of assistance to us in avoiding upward pressure.

If neither the forceps nor the crotchet be deemed capable of effecting delivery our next alternative is *turning*. The particular circumstance which generally obliges us to resort to this mode of delivery is the partial or complete recession of the child out of the vagina. Should the entire fetus have passed into the belly, it may still be followed by the hand, and extracted without any great difficulty, though much care is requisite to avoid drawing down along with it some of the intestine or omentum. This caution is the more needful if the rent be situated in the posterior part of the vagina. Though several hours may have elapsed from the moment of the laceration we are not, on this account, to be deterred from pursuing the fetus with the hand, and endeavouring to withdraw it through the rent, as such a proceeding is not only warrantable but generally quite feasible.

For example, in a case of my own there was an interval of four hours between the occurrence of the accident and the performance of turning; in Mr. Ross' case there was a like interval (this patient recovered, moreover); in a case of Collins' the interval was twelve hours; and in Mr. Goldson's case it reached to twenty-six hours; yet in none of these instances was difficulty experienced in seizing and bringing down the fetus out of the peritoneal cavity and through the vaginal laceration. Where the child has slipped into the belly, through a rupture exclusively confined to the uterine structure, the same facility in turning is not to be expected, because with the contraction of the womb the rupture will undergo a proportionate diminution in its size.

Baudelocque, with his accustomed shrewdness, has noticed this practical difference between uterine and vaginal lacerations; and certainly the point is one well deserving of attention.

If the child have been so many hours in the belly that its extraction *per vias naturales* is impossible, or possible only by doing great violence to the soft structures, then the easier and less dangerous course would be to remove the child by gastrotomy; or, even though no unreasonable length of time had elapsed since the recession of the fetus into the peritoneal cavity, yet if the patient had a contracted pelvis, and was come to the full time, I am convinced it would be giving her a better chance of recovery to withdraw the fetus by the abdominal section than to subject her to the tedious and troublesome business of delivering it according to

the ordinary method of turning. In giving this qualified sanction to gastrotomy I am justified by the numerous recoveries which have followed the operation when performed for rupture of the uterus; in fact a larger proportion of cures have resulted from this than from any other mode of delivery; and I cannot suppose it would be less successful after laceration of the vagina, though I am sure it would not be so often necessary.

Two successful cases of gastrotomy, after rupture of the uterus or vagina, have been recently published—one by Dr. Dyer (*British Medical Journal* for September 9, 1865), where the interval between the occurrence of the rupture and the extraction of the child was four hours and a-half; and in the other case, related by Dr. Crighton (*Edinburgh Medical Journal*, 1864), the interval was fourteen hours. In neither case was there any attempt made to withdraw the child by turning.[a]

Dr. Murphy strongly advocates gastrotomy in all cases of rupture where the child has escaped into the belly; but I cannot altogether agree with him that "when the child is in the cavity of the abdomen, forced thither by the uterus or by the hand of the practitioner, the only operation that appears to give a reasonable chance of success is gastrotomy." This opinion may hold good if there be pelvic deformity, or if the seat of rupture be the body or fundus of the uterus; but it is not tenable, I think, if the rupture be in the vagina or os uteri, and that the pelvic capacity is unabridged.

I have before remarked that the placenta sometimes follows the child through the vaginal rent into the general cavity of the abdomen. Where this has occurred its artificial extraction should be effected immediately after that of the child. In doing this the excellent rule of following the cord as our guide to the placenta should be implicitly attended to. On the other hand, if the placenta remain still in utero, we may ordinarily wait for ten or fifteen minutes, to allow time for the natural efforts to expel it. Should

[a] A deeply interesting question here suggests itself. The recoveries after gastrotomy (for ruptured uterus or vagina) greatly exceed the recoveries from Cesarean section, which is quite contrary to what we might, *à priori*, have supposed. How is this difference to be explained? The gastrotomy group of cases, as a whole, probably possessed somewhat better constitutions; but surely this is not sufficient. Herein, I think, lies the cause of difference—the situation and direction of the artificial opening in the genital canal. In the one category the opening is longitudinal, and is situated in the fundus and body of the uterus; in the other category it is circular or oblique, and occupies the vagina or neck of the womb.

it not then come away, however, and that gentle pressure on the uterus, with traction of the cord, fails to dislodge it, our best course, I believe, is at once to set about extracting it manually. It may be worth while remarking that I have never but once found it necessary, in any case of ruptured uterus or vagina, to pass the hand into the uterine cavity for the purpose of removing the after-birth; but I can readily understand that such an operation will require extreme care and gentleness in its performance, if we would avoid increasing the damage that the soft parts have already sustained.

The placenta having come away, we can now ascertain, if we have not already done so, the exact situation and extent of the laceration. At the same time we should make sure that no portion of gut has prolapsed through the rent. We should next endeavour to place the edges of the laceration as accurately in contact as can be done under the circumstances.

It can scarcely be doubted but that the permanent retaining of the torn parts in apposition would conduce to the chances of the patient's recovery; and, with the proper instruments, I really think there could not be any great difficulty (the vagina being so relaxed and capacious) in making two or three—not more—stitches with wire suture, just to hold the edges of the laceration together.[a]

Of course, I fully concede that the circumstances under which this accident commonly occurs are such as to preclude the possibility of attempting this. But I need not remind you that our object should be to find out what is the utmost art can do to remedy this accident; and, knowing this, our endeavour should be to carry it out in practice as far as circumstances will admit.

[a] Dr. Marion Sims relates (in his "Clinical Notes on Uterine Surgery") two cases which have a close bearing on the subject before us, as in each the peritoneal cavity was opened during operations upon the uterus. In one case the opening was through the cervix, and "would easily have admitted the passage of three fingers at a time into the peritoneal cavity" (p. 133). In the other case the chain of the écraseur made "an immense hole of a semilunar form, in the *cul-de-sac* of the vagina, through which we could look for three or four inches up into the peritoneal cavity, and observe the movements of the viscera with every respiratory act" (*op. cit.*, p. 207). Now, in each of these instances he closed the opening by bringing the edges together with metallic sutures, with as little delay as possible, and *both patients recovered.* In the former case he carefully removed, by means of sponge probangs, the blood which had forced its way into the cavity of the peritoneum, and closed the wound completely by five or six points of suture.—This woman "recovered rapidly." In cases of lacerated vagina or cervix I do not see why the same means should not be used to remove the blood which may have got into the serous cavity, and whose presence there must exercise a pernicious influence.

The soundness of the principle on which the above suggestion is based can hardly be disputed. Whenever a serous cavity is penetrated, whether accidentally or intentionally, is not the first step towards effecting a cure to carefully close the unnatural opening, and by union of the wound to permanently seal up the cavity again?

The passing of the sutures could not occupy many minutes, neither could this proceeding add to the existing dangers. Practically, the only difficulty I see in the way of carrying out the suggestion, supposing we have the necessary instruments at hand, is how to command a sufficiency of light, as of course the patient cannot be moved; but this difficulty is not one of an insuperable kind.

The exhibition of stimulants is usually needful during and immediately after delivery; nor can we safely suspend their use until decided symptoms of reaction begin to show themselves.

The subsequent treatment it is needless for me to describe, as it is essentially the same as that required in rupture of the uterus. The main points to be attended to, I think, are these, viz.:—

1. To enjoin absolute quietude of body. The urine should be drawn off with the catheter, to save the disturbance which the use of the bed-pan would cause.

2. The opium treatment should be fully carried out.

3. Nourishment, in the form of beef-tea, strong mutton-tea, or chicken-tea, should be given in small quantities, and at short intervals.

4. If the symptoms of peritoneal inflammation become developed, I would apply leeches over the seat of the greatest tenderness, in numbers proportionate to the patient's strength; and would employ mercury externally and, with due care, internally.

5. The utmost caution and reserve should be used with regard to purgative medicine. Some days should be allowed to pass before any attempt be made to procure an evacuation from the bowels.

Where a patient escapes the more immediate dangers arising from the nervous shock, and from peritoneal inflammation, there is, at a later period, yet another danger to which she is exposed, and that is pelvic abscess, behind or to either side of the uterus, and in connexion with the injured structures.[a] I do not know whether

[a] This danger seems to have entirely escaped the notice of systematic writers upon lacerations of the uterus and vagina. Nevertheless a careful perusal of the clinical records of this accident will serve to convince one of its existence.

this result ever follows ruptures of the uterus. All the examples I have found of it were cases of laceration of the vagina; and I can well suppose that it would not be at all so likely to supervene upon a rupture of the fundus or body of the uterus. In some, if not in most instances, we may attribute this abscess to the entrance of blood and air into the peritoneal sac; whereupon, as happens in certain exceptional cases of idiopathic pelvic hematocele, the cyst which forms around the effused blood takes on suppurative action. In the idiopathic or spontaneous pelvic hematocele suppuration is extremely rare; whereas in the traumatic pelvic hematocele, that, namely, which results from vaginal laceration, suppuration is not uncommon, and is to be accounted for partly by the presence of the rent in the vagina, and partly by the presence of atmospheric air along with the extravasated blood.

Pelvic abscess formed in three of Collins' cases of laceration of the vagina. One of these cases recovered and two died, and in each of these it was ascertained at the *necropsy* that the psoas muscle was more or less involved in the abscess. One patient died on the sixth and the other on the twenty-sixth day. When I was an assistant at the Lying-in Hospital I saw a patient who, on the forty-second day after a rupture of the vagina or cervix, died in consequence of an extensive abscess at the left side of the pelvis, extending upwards to nearly as high as the origin of the psoas muscle.

A recovery from laceration of the vagina and cervix occurred under my own care, at the Lying-in Hospital, in 1860, the details of which Dr. Byrne read to the society; and in this instance a very offensive puriform discharge was observed flowing from the vagina on the twelfth and succeeding days.[a]

A case is related by Collins which clearly shows the possibility of internal secondary hemorrhage occurring. The laceration was situated "anteriorly, at the union of the uterus and vagina." The woman sank on the ninth day, and at the *autopsy* the immediate cause of her death was found to be hemorrhage into the abdominal cavity. A slight discharge of blood from the vagina had shown itself some hours before dissolution.

In my sixth case (already related) it may be remembered that on

[a] I have not included this case amongst the examples of laceration of the vagina as the full extent of the tear was not ascertained, although it is certain the vagina was chiefly, if not exclusively, implicated.—See Dr. Byrne's report of the case in Vol. xxxiii. of this Journal.

the fourth day, after a movement of the bowels, produced by the enema, a considerable quantity of grumous blood was discharged from the vagina. This, I have no doubt, came from the peritoneal cavity, as a great deal of the like fluid was found in it after death. Most probably, however, all this blood had been extravasated a short time subsequently to the laceration. Although not exactly in point, still the clinical fact is worthy of note.

I shall now briefly recapitulate the principal points in respect to which laceration of the vagina or os uteri seems to differ, at all events in some degree, from rupture of the cervix or body of the uterus:—

1. Premonitory symptoms are very rare.

2. The immediate constitutional effect of laceration of the vagina is not, on the whole, so profound as that arising from rupture of the uterus.

3. Vomiting is occasionally a symptom of the accident, but it is not of the *coffee-grounds* character.

4. The laceration is very rarely, if ever, induced by deformity of the pelvic brim.

5. The head is commonly engaged in the pelvis at the time the laceration occurs.

6. The tear can in no way be attributed to contractions of the structure directly involved.

7. The laceration almost always takes a circular direction, and

8. Remains patulous, or at least shows very little disposition to contract.

9. The escape of the fetus into the peritoneal cavity follows more frequently upon vaginal laceration than upon uterine rupture.

10. The escape of the placenta, likewise, through the laceration is more apt to take place here than in ruptures of the uterus.

11. Prolapse of the intestine, also, is a less rare complication of vaginal laceration than of uterine rupture.

12. The operation of turning is found to be practicable for a longer period after laceration of the vagina than of the uterus.

The four preceding characteristics (Nos. 9, 10, 11, 12) naturally result from the peculiarity stated in No. 8, which belongs to lacerations of the vagina or os uteri.

13. There is a greater liability to pelvic abscess after vaginal laceration; and this we may, in some degree, attribute to the greater likelihood of atmospheric air entering the belly through the solution of continuity in the vaginal canal.

14. Lastly, a comparison of the mortality of these two lesions clearly proves that vaginal laceration is a much less fatal accident than uterine rupture.[a]

ART. XII.—*Notes in Medicine and Surgery.*—V. By PHILIP CRAMPTON SMYLY, M.D., Univ., Dub.; F.R.C.S.I.; L.C.P.I.; Surgeon to the Meath Hospital, and to the Institution for Sick Children, Pitt-street.

ASCARIS LUMBRICOIDES IN THE TRACHEA.

PATRICK F., aged three and a-half, a fine healthy-looking boy, was admitted into the Meath Hospital on Friday, 9th February, 1866, on account of great difficulty of breathing.

The child's sister, who brought him to the hospital, gave the following account:—He had been in perfect health until dinner time, about four o'clock. She left the room for a few minutes, when on her return some one exclaimed "the child is choking!" She thought a piece of cake the child had been eating had stuck in the throat. Failing to get it down she at once brought him to a doctor some three miles off. He tried to push down the supposed obstruction with an instrument, but failing to relieve the breathing, he sent him to hospital.

Mr. Owens, on admission, endeavoured to remove the obstruction by introducing his finger two or three times, and he also used a probang. The symptoms of suffocation becoming every moment more intense he sent for me.

The child was in a state of complete anesthesia; the eyes were half closed, and beginning to glaze; the respirations were very laboured; he was apparently dying from some obstruction at the top of the windpipe. From the hurried history I received I concluded that the child had taken boiling water. The girl had not been in the room when the child began to choke. The symptoms had become rapidly and steadily aggravated; while the finger had failed to detect, and the probang to remove the supposed cause of the obstruction.

[a] At the Meeting of the Obstetrical Society following that at which the above paper was read, Dr. J. A. Byrne communicated the particulars of a case of extensive laceration of the vagina, ending in recovery. It very well exemplified all the leading points above laid down as being characteristic of vaginal laceration. The history of this important case will doubtless be published.

I passed my finger, with some difficulty, between the teeth into the pharynx, when it met a soft spongy mass On pressing through this the finger distinctly entered the larynx. I concluded that this soft mass was œdema, the steady increase of which had given rise to the steady increase of the symptoms.

I proceeded to prepare the instruments for laryngotomy, but on returning to the ward I found that the child had ceased to breathe for some minutes; there was no pulse at the wrist; the face had become livid, and the eyes were quite glazed. Having among my instruments a catheter open at both ends, I passed it at once into the larynx, guided by my first finger, and pushed it on into the trachea; but without effect. Artificial respiration was then performed; I filled the chest by blowing through the catheter, and emptied it again by pressing with one hand at either side over the false ribs. This process was continued for more than twenty minutes before the child gave a gasp, and shortly after Professor Macnamara, who held the wrist, reported that he felt the pulse returning. After three quarters of an hour respiration was fully established through the catheter. The pulse became full at both wrists, and the respiratory murmur audible over the chest.

A consultation was then held whether laryngotomy should be performed or the catheter be left in the trachea.

The operation was decided on, and was performed at once. The breathing became much embarrassed when the catheter was removed, though the air passed freely through the tracheal tube. Shortly after the operation the child seemed to be sinking; some brandy and water by the mouth, and some brandy and turpentine by the rectum revived it again. For some hours it breathed calmly through the tube. Convulsions, however, set in during the night, the attacks increasing in violence and frequency until about eight o'clock on Saturday morning. The intervals then became longer, and the attacks less violent until four o'clock on Sunday morning. The pulse then began to fail, and the child died two hours afterwards.

Respiration was performed entirely through the tracheal tube until Saturday mid-day, when air was expired through the mouth and nose as well as through the tube. The child swallowed beef-tea and wine without difficulty during the thirty-six hours he was in hospital.

On Monday morning the *post mortem* was performed by Mr. Owens, the resident. When the tongue was drawn down, after detaching the structures from the lower jaw, the pharynx was found filled with a mass of round worms. The parts were then

removed together; and on laying open the œsophagus, so as to see the top of the windpipe; no œdema was discovered, only slight traces of inflammation about the epiglottis and arytenoids. Protruding, however, from the glottis was the head of a round worm. See Fig. 1. The preparation was then turned round, and the trachea divided from the opening made at the time of the operation down through the bronchial tubes. This section brought to view the rest of the worm. It lay with its head protruded from the larynx, the tail just below the vocal cords, and the lap resting on the bifurcation. See Fig 2. Both drawings were made by Mr. Burnside, Mr. Lewis' talented artist, from the preparation mounted by my friend Dr. Barker for the Museum of the Royal College of Surgeons.

There were no worms in any other part of the body though all the organs were examined.

"Royal College of Surgeons, Ireland,
"Feb. 13, 1866.

"My Dear Sir,—The preparation you brought to the college yesterday is remarkable for the number of entozoa, the position in which they were found, and the fatal result. There were six specimens of the ascaris lumbricoides—two of them males. Yours most truly,

"John Barker."

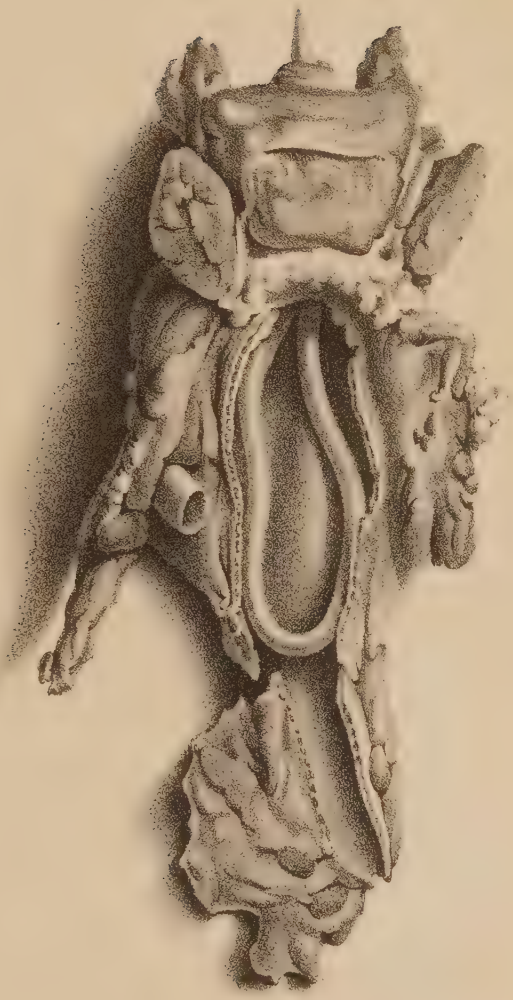
The operation of tubing the laxynx in cases of obstruction at the top of the windpipe had been suggested to me about two years ago by my friend Dr. O'Flaherty. He had not himself performed the operation, but pointed out the advantages of the proceeding in such cases. I determined to try it whenever a suitable case occurred, and accordingly placed one of Hutton's catheters with my instruments for tracheotomy.

Since the operation I have found that M. Bouchut has recommended "tubing the glottis as a substitute for tracheotomy in croup, and leaving for a time in the orifice a metallic ring." In Art. 5551 of the *Journal of Practical Med. and Surg.*, 1st year, English edition, the editor concludes in the following words:—"All that can be said—and M. Bouchut has kindly permitted us to witness the operation—is that the tubing the larynx is not a difficult process, that the canula remaining in the glottis for thirty-six hours was perfectly harmless; that the two children could speak distinctly, and take liquids without swallowing them the wrong

PLATE I.

Dr P C. SMYLY

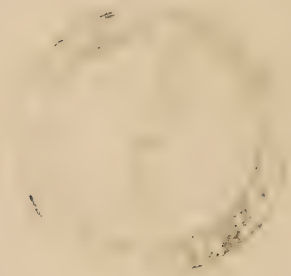
Ascaris Lumbricoides, head protruding from the Larynx,
Asophagus laid open



DR. P. C. SMYLY.

Ascaris Lumbricoides in the Trachea.

J. Lewis Lith. Dublin.



way; and that there was in every respect a temporary improvement analogous to that which follows tracheotomy."

It is very rare to find a lumbricus in the trachea, though worms are not unfrequently found in the mouth after death, and sometimes during life.

After death worms almost always leave the body. A gentleman lately returned from Penang told me that in Malay, where murders are frequently committed by cutting the throat, the horror of the scene is greatly increased by numbers of round worms leaving the body through the gash, and wriggling and writhing in the blood.

In fevers and other diseases worms leave the body, and "after long periods of abstinence or inanition they have been found to migrate and escape from the anus, mouth, and nose."[a]

Sometimes they leave the body without any apparent cause. My friend Dr. Robert White has in his possession a number of round worms which a woman removed from the mouth of a child otherwise in perfect health.

In most of the general treatises on medicine the fact is stated that worms have been found in the larynx and trachea.

Copland says:—"When the worm passes into the stomach, it may rise rapidly into the upper part of the œsophagus, or into the pharynx, and even get entangled in the larynx, and produce the most distressing effects. But such occurrences, although observed, are very rare."[b]

Neimeyer says:—"Der spulwurm wohnt im Dünn—und Dickdarm, macht aber von dort aus weite wanderungen und kann dabei in den magen, in den œsophagus, selbst in den larynx gelangen."

The only parallel case I can find is recorded in Andral's *Path. Anat.*, p. 210:—

"These worms sometimes quit the alimentary canal, and make their way into different neighbouring parts. M. Blandin and I once found, at the *Hôpital des Enfans*, an ascaris in the cavity of the larynx; it occupied the space between the cordæ vocales, and part of the body was still in the pharynx. The child had been suddenly attacked with extreme dyspnea, and died very shortly after in a state of asphyxia."

Dr. E. P. Wright very kindly wrote me the following note:—

[a] Copland's Dic., p. 1,405.
[b] Copland's Dic., p. 1,405.

"Dear Smyly,—I have hunted up what has been written on the subject of nematoid worms and can only find one record of a case at all like yours, viz.:—J. H. Cutler—death by worms—a large lumbricus being found in the windpipe."—*Boston Med. and Surg. Journal*, Vol. LXVI., p. 392, 1862.

Dr. Robert Smith and Mr. William Stokes have kindly called my attention to a case reported in the *Trans. of the Dub. Path. Soc.*:—

"Dr. Law presented a specimen of the ascaris lumbricoides, about a foot in length, and apparently full of ova, passed by the mouth, by a female, aged 22 years, while labouring under typhus fever. In the evening of that day she had a violent attack of coughing: the nurse, supposing that she was choking from a collection of mucus in the mouth and throat, came to her relief, and drew out of her mouth the worm now exhibited. It can easily be imagined that the specimen now exhibited was either *en route* towards or had already made some way into the trachea, from which it was expelled by coughing."

There is no mention of this accident in any of the more recent works that I have been able to consult.

Art. XIII.—*Cerebro-Spinal Meningitis.* By Robert Law, M.D., Professor of Institutes of Medicine in School of Physic.

Early in the last year we were alarmed by the accounts of a disease which had appeared in different localities through Europe, but chiefly in Russia and Prussia. This disease was variously described. It was supposed to have some of the characters even of the plague. It was thought to be one of the various forms of low asthenic fever with which we have been so familiar in latter years, many of which, although not exactly similar, had such a strong family likeness that it might be said—*facies non omnibus una, haud diversa tamen.* The fever was said to resemble, in many particulars, that which prevailed in this country after the year of scarceness, and which got the designation of famine fever. Again, another form that this disease assumed was that of cerebro-spinal arachnitis, a formidable disease, of which we had a few instances,

but these of a very severe and fatal character. The Editor of the *Dublin Medical Journal*, sensible of the benefit that had accrued to medical science from the valuable reports of Barker and Cheyne on the fever that had prevailed in this country in the years 1817, '18, '19, when the famine fever was on the decline, invited the medical men who had had opportunities of witnessing the character and treatment of the disease to furnish him with the results of their experience, in order to publish them in the Journal, that they might become a permanent record. Amongst the communications supplied to the Journal, I contributed my contingent, which is in No. XVI., Nov., 1849. In my observations, I remarked:—"The most common and loudest complaint of our patients was an aggravation of what the subject of fever generally describes as pains in the bones, but which really means pains in the course of the spinal nerves. While in former fevers this complaint was seldom more than that of a contused or bruised feel, or of such a sense of discomfort or fidgety restlessness as the French so significantly express by the term *malaise*, here the individual in many cases seemed to suffer as intensely as in the severest cases of acute rheumatism. These pains were more or less general in different patients. In some they affected the back of the head and neck,; in some they only ran down the legs; while in others they spread themselves through the whole body, and embracing the sides, imparted the sensation of painful constriction. The nape of the neck and across the loins were the points to which the patients most frequently referred their pain. When it affected the former position, which it did in many cases, we regarded it with some degree of apprehension, from this being the starting-point of that formidable disease, cerebro-spinal meningitis, into which we are sure it would have passed had it not been arrested by treatment." The reason for our transcribing these observations is that they exactly describe cases which presented themselves to us in the course of the last Summer, and which seem to us to deserve the designation of cerebro-spinal meningitis. Although the majority of the cases that came under our notice were females, and with these assumed the features of aggravated spinal irritation, the two first cases were young men. The first was a young man, aged twenty-one, who had been in a grocer's establishment. He was admitted into the hospital. All his members seemed to be paralysed. He could move neither arms nor legs. He had a vacant stare, pupils dilated, nor could he understand anything that was said to him. There was no abnormal heat of skin. He twisted

his body, and had a general fidgetiness, so that he required to have a person constantly beside his bed to prevent his wriggling himself out of it. Pulse not frequent, but small and weak. Although the weather was unusually warm, his hands had the appearance of being affected with chilblains. The legs exhibited a mottled congested appearance. We could obtain no satisfactory account of the patient's antecedents, either before his illness or from its commencement, to the time of his admission into hospital.

We directed leeches to be applied behind the ears and along the spine, turpentine stupes to the legs, and to have the following mixture:—Sesqui-carbonate of ammonia, one drachm; Hoffman's anodyne liquor, two drachms; camphor-water to eight ounces. An ounce to be taken thrice daily.

He now became very noisy and excited, appearing to awaken, as it were, from a stupor. We then ordered for him the following mixture:—Tartar emetic, one grain; Battley's sedative liquor, thirty-five minims; spirit of nitrous ether, half an ounce; camphor-water to eight ounces. An ounce to be taken thrice daily.

He took but two doses of this mixture when he became quite calm; he was no longer noisy, but exhibited a stupid, vacant expression. He was extremely weak, and had hardly any use of either his arms or legs. We allowed him wine liberally, and hot tea, and had a stimulating liniment, rubbed along the spine, and turpentine stupes to the legs. We afterwards ordered him the following mixture:—Sesqui-carbonate of ammonia, one drachm; Hoffman's sedative liquor, half an ounce; infusion of arnica, three ounces; camphor-water, to eight ounces.

He improved but very slowly, and ultimately got quite well; and when well he had so different an appearance from what he had when he first came under our care that we should have questioned his identity. He was about a month in hospital.

The next case was that of John Keogh, aged nineteen, who was equally helpless as to the use of either legs or arms, and had to be carried into the hospital. He was literally in torture from pain in his temples, forehead, and spine, and extending into legs and arms. The pulse was not abnormally quick, nor was the skin unusually hot, but his exhaustion seemed to be extreme. We directed a blister, sprinkled with two grains of morphine, to be applied to the forehead, and leeches along the spine. The blisters to the forehead brought very little relief, but the leeches nearly removed the pain

from the parts to which they were applied. We now directed leeches to the temples, and they were as successful in removing the pain in forehead and temples as were the others in removing those of the spine. It was most striking the change of expression that ensued almost immediately after the leeches had been applied; the countenance lost the look of anguish that it had before. We ordered an anodyne liniment, composed of camphor-liniment, chloroform-liniment, and tincture of opium, in equal parts, to be rubbed along the spine, and directed the following mixture:—Sesquicarbonate of ammonia, one drachm; Hoffman's anodyne liquor, two drachms; infusion of arnica, three ounces; camphor-water to eight ounces. An ounce to be taken thrice daily.

His pains soon entirely disappeared, but he was very long recovering the use of his limbs. His debility was very great. He was a month in hospital before he recovered his strength.

Margaret Kennedy, aged sixteen, admitted into hospital June the 16th, 1865. She had been five days ill before her admission into hospital. Her illness began with shivering and pains all through her. She now complained of her back and of most distressing pains through her arms, and legs, and sides, as if she were tightly bound round with cords. She experiences great pain when the spine is pressed even slightly in the lumbar region. She feels extremely weak and exhausted. Stomach very irritible. Tongue loaded. Abdomen tympanitic. She was ordered cold lotion to her forehead and leeches to the portion of the spine that was painful when pressed, and the following mixture, with the view to quiet the irritability of the stomach:—Acetate of lead, ten grains; acetate of morphia, one grain; vinegar, two drachms; syrup, six drachms; water to eight ounces. An ounce to be taken thrice daily.

She was ordered four ounces of brandy. Our report was:—Head relieved. Sickness of stomach ceased. Much less pain in the back and through the limbs. We directed the following liniment:—Compound camphor liniment, two ounces; aconite liniment, tincture of opium, of each an ounce. This liniment to be rubbed along the spine.

She slightly improved from day to day, the pains in the limbs becoming less, but when they became less in one part they invaded another. The mammæ, which were unusually developed in so young a person, became their seat in a very aggravated degree, but

were relieved by stupes. The tenderness on pressure of the spine continuing, a belladonna plaster afforded some relief. Although the pains of the limbs lost a good deal of their severity, they were still very distressing.

She complained much of oppression of the chest, and had a good deal of bronchitis.

She had a very tedious convalescence. Her weakness was extreme. She was very long before she could use her limbs.

Ellen Holohan, aged twenty-two, unmarried, admitted into hospital June 30, complaining of severe pain in temples and forehead, and root of the nose; also in back and in arms and legs. She had no fever, but great weakness and loss of appetite. She stated that she had been three weeks ill, and that her illness began with shivering and a feeling of cold water trickling down her back. She then became hot, and then had perspirations, and then had pains in her back. This set of symptoms did not recur, but she was extremely weak; yet she continued up and about, until, as she said, every member but her tongue became paralysed. Eight leeches were directed to be applied along the spine, cold lotion to the forehead, warm stupes to the legs, and to have the following mixture:—Sesqui-carbonate of ammonia, one drachm; Hoffman's anodyne liquor, two drachms; camphor-water to eight ounces. An ounce to be taken thrice daily.

July 2nd—Pains relieved.

3rd—Has a good deal of pain in the forehead and root of nose, and in the nape of neck; pains in back and legs much lessened. Four leeches to be applied to the nape of neck. Other things to be continued.

5th—Pain in back of neck gone, those of back and legs much relieved. Liniment to back, and stupes to the legs to be continued, She continued to improve, and no note was taken till the 9th, when she complained of distressing acidity of stomach and of a strong pulsation in the epigastrium, for which we directed the following mixture:—Liquor of potash, a drachm; compound tincture of gentian, two drachms; syrup of orange-peel, five drachms; infusion of colombo to six ounces. An ounce to be taken thrice daily.

She now gradually but very slowly recovered her strength, and left the hospital on the 21st.

One of the most remarkable features of this case was the striking

improvement in the expression of the countenance that took place as her symptoms gave way. I attach importance also to the derangement of the stomach, indicated by acidity, as I had occasion to observe the same symptom to occur to other persons at the same time, while they only complained of this with a sense of unusual sickness and exhaustion. At the time Holohan was admitted into the hospital I had there under my care her two brothers and two sisters, all affected with fever. The fever under which the two sisters and one brother laboured was comparatively mild, but that of the elder brother, aged seventeen, although mild at first, assumed a very alarming character in its progress, exhibiting symptoms of high cerebral excitement, which passed into profound coma, from which, happily, he was roused by a large blister applied to the vertex. As the blister rose he seemed as it were to awaken out of a deep sleep. The skin in this youth's case was bathed in perspiration, and afterwards thickly covered with sudamina. I would here incidentally allude to the great advantage derived from the application of a blister to the vertex in cases of either actual or threatening coma supervening on high cerebral excitement. We have observed in many cases, as in the one just recorded, how, when the blister began to rise, the patient seemed to awaken, as it were, from a deep sleep.

At the same time that Holohan and Kennedy were in hospital, there were three sisters—Bridget Fannin, aged twenty-two; Ellen Fannin, aged twenty; and Margaret Fannin, aged nineteen—also under my care in hospital, exhibiting somewhat similar symptoms. The eldest and youngest were admitted on the 3rd of July, supposed to be cases of fever. When they were visited the day after their admission they did not exhibit symptoms of fever, the younger especially only complained of not being well, but could hardly say what was the matter with her. She only remained with us three days, exhibiting from the first an impatience of being in hospital, which we learned afterwards was owing to a matrimonial speculation which she feared her illness would interfere with. The elder sister's illness was of a much more serious character. She complained of most distressing pain in her temples, her forehead, and all through her head, also in her back, along the spine, which was especially sensitive to even slight pressure, and of pains in both legs and arms. Her countenance bespoke the most intense suffering. The skin was not abnormally hot. The pulse was above 110 in the minute. Her prostration

was extreme. Two leeches were applied to each temple, and eight along the spine. The legs were stuped. She was ordered six ounces of wine. These means afforded some relief, but her pains both in head, and back, and legs were still very distressing. In a few days twelve more leeches were applied along the back, and the stupes to the limbs, which soothed her very much. The following liniment also was rubbed along the spine, in the interspaces between the leech-bites:—Compound camphor liniment, three ounces; tincture of aconite and chloroform, of each half an ounce

The leeches always brought relief. She still complained of pain in the forehead, for which a blister was applied there. Her extreme prostration made us increase the quantity of wine, and also to allow her some brandy. The blister relieved the pain of the forehead, but the pain in the back and legs still continuing, twelve more leeches were applied, and the stupes continued. On the 14th we pronounced her convalescent, her pains being gone, but she was extremely weak, and felt as if she had not a leg to stand on. She was very deaf. We directed for her the following mixture:—Sesqui-carbonate of ammonia, one drachm; infusion of arnica, two ounces; tincture of orange-peel, half an ounce; decoction of bark to eight ounces. An ounce to be taken thrice daily.

Her convalescence was very tedious. She was very slow in recovering her strength. Her stay in the hospital was altogether a month.

On the 8th of July the youngest sister, who had been under our care for fever, but left us after three days, was readmitted, accompanied by her second sister, Ellen Fannin. The former complained of most distressing pains in her legs, and of extreme sickness. Her countenance expressed deep suffering. Her appearance in this respect differed much from what it was before when she was with us. The second sister, Ellen, complained of most distressing pains at the lower part of the back and in hypogastrium, which was most sensitive to the slightest pressure—nay, rather to the touch. Pressure was not only more tolerable, but even afforded relief. There was considerable tympanitic distention of the abdomen, and a tumour could be felt above the pubes. From the coincident symptoms we regarded this tumour to be what is designated a phantom tumour, and were sure that it would disappear with the other symptoms. She complained of the same distressing weakness and exhaustion as her sister. For her we directed the following

mixture:—Spirit of chloroform, two drachms; camphor-water to six ounces. An ounce to be taken thrice daily.

Our reason for this prescription was our experience of its value in hysterical tympanitis. We also directed the abdomen to be fomented with camomile and poppy-heads, and that she should have four ounces of wine. For her sister, who only complained of pain in her legs, we only directed the legs to be stuped, and that she should have four ounces of wine. The stupes always relieved her pains for the time, but they continued to return for some days, and finally ceased. The means employed with Ellen Fannin relieved the pain of back and hypogastrium, but there was still considerable tympanitic distention of the abdomen, for which we directed turpentine stupes, and the following pills:—Compound galbanum pill, twelve grains; sulphate of quinine, four grains; extract of nux vomica, two grains; to be made into four pills. One to be taken thrice daily.

My reason for making quinine an ingredient in my prescription was that I regarded the disease to be a kind of malarious fever, especially affecting the spinal nerves, but not confining its influence to these, but involving the brain and also the ganglionic nerves. The tympanitis was somewhat relieved, but she now was affected with most distressing irritability of stomach. The only thing it retained was wine; everything else it rejected. We directed for her effervescing draughts, with laudanum. They did not succeed; nor did a blister, sprinkled with two grains of acetate of morphia, applied to the epigastrium. We then directed the following mixture:—Liquor of potash, one drachm; dilute hydrocyanic acid, twelve minims; liquor of hydrochlorate of morphia, half a drachm; tincture of orange-peel, three drachms; infusion of mint to eight ounces. An ounce to be taken thrice daily.

Her stomach now became settled, but she was extremely weak from this organ so long rejecting food in every shape. We had ordered her enemata of beef tea and brandy—an expedient to which we had resorted with great success in cases where a weak stomach could not do as much as the failing strength of the patient required. We have a vivid recollection of one interesting case where the pulse had ceased and the power of deglutition was gone, and when from such enemata ebbing life seemed to return. Nor have these instances been few. In the present case the stomach soon was able to take a due supply of food, so that we had no occasion to resort to this method. Her pains had now ceased, and we had now only to deal with the

weakness, which was extreme. The youngest sister, who only complained of pains in her legs, and which yielded to continued stuping, exhibited the same prostration, and also then became quite deaf. They all felt, as it were, a sense of overwhelming lassitude and fatigue amounting almost to that of paralysis.

They were extremely slow in recovering their appetites, and could only take wine or brandy; while the illness of the eldest, which was much more severe than that of the other two, and required more active treatment, or to which attack more active treatment was applied, as she had thirty leeches to her head and back, lasted a month; theirs lasted three weeks: still the cure of the eldest appeared to be more complete, and she appeared to recover her strength sooner than her sisters.

I learned that the father of these young women had died about a month before their illness; and the account that I got of his illness was that he had a kind of fever which ended fatally in an attack of apoplexy, attended with hemiplegia. He had been a man of very intemperate habits. We think we can discover a kindred relation between the illness of the parent and the children, the same morbific influence acting on both, but modified in its action on the parent by his previous habits.

Anne Walsh, aged forty, married, admitted into hospital July 21, complaining of most distressing pains in the forehead, temples, down her back and through her arms, down to the very tips of her fingers, and down her legs, even to her toes. She said she felt as if her feet were asleep. She described her sense of sickness and prostration as extreme, which her expression seemed fully to confirm. We directed leeches to be applied along the spine; a blister, sprinkled with two grains of sulphate of quinine, to the forehead, and that she should have two grains of quinine made into a bolus with aromatic confection, three times daily; and an ounce of the following mixture after each bolus:—Sesqui-carbonate of ammonia, one drachm; Hoffman's anodyne liquor, two drachms; camphor-water, eight ounces.

22nd. Pains of limbs much relieved, but pain of head as distressing as ever, both front and back; two leeches were directed to be applied to each temple, and six to the nape of the neck.

23rd. Head is entirely free from pain—in fact, all her pains have left her. Her extreme prostration, and that she got no sleep at night, were now her only complaint; for the latter we directed a hypnotic

draught of five-and-twenty drops of Battley's sedative, and for the former continued the boluses of sulphate of quinine, with the mixture of ammonia, Hoffman's anodyne, and camphor-water. We also gave her a liberal allowance of wine. She recovered her rest, and gradually improved in strength, so that on the 1st of August we pronounced her well, she then only complaining of noise in her ears—a symptom of which almost all patients affected like her complained, some more and some less.

Jane Nugent, aged twenty-eight, admitted into hospital July 19, complaining of pain in back and stomach. Has no headache. Skin hot, and densely spotted with a bright, florid efflorescence. Pulse 120—soft. Tongue red at point and edges. Complains of great prostration. Feels considerable pain when spine is pressed.

Ordered bolus of three grains of sulphate of quinine, one grain of antimonial powder, and of aromatic confection, q. s., thrice daily; and the following mixture:—Sesqui-carbonate of ammonia, a drachm; Hoffman's anodyne liquor, two drachms; camphor-water to eight ounces—an ounce to be taken thrice daily. And the following liniment to be rubbed along the spine:—Compound camphor liniment, three ounces; chloroform and tincture of aconite, of each half an ounce.

20th. Experienced great relief from having her back rubbed with the liniment.

23rd. Abdomen very thickly spotted with bright, measly efflorescence, rather tympanitic, with tenderness on pressing the iliocecal region. Diarrhea, with yellow discharges. To have five grains of aromatic chalk powder with opium, thrice daily.

24th. Diarrhea has almost ceased. No tenderness on pressing the abdomen. Pain of the back not quite gone. Liniment to be continued.

She now had some delirium, which we attributed to the sudden ceasing of the diarrhea. Having frequently observed in typhoid fever cerebral disturbances to supervene on the diarrhea being checked, we are always on our guard, and take precautions against it by cold lotion to the forehead, and fomentations to the legs. These means succeeded in the present instance. She now went on favourably, and on the 31st we pronounced her convalescent, although extremely weak.

This was the only case of this group in which there was what could be called true typhoid fever. We made the experiment of

treating it with quinine and antimonial powder, regarding it as a kind of malarious fever, but when the diarrhea came on we did not feel ourselves justified in pushing this treatment further.

Master Ormsby, aged twelve, had been ill with sore throat and swelling of the glands of the neck, and severe pains in his limbs, with extreme prostration of strength. He felt utterly knocked down, while he had very little fever. We prescribed for him some cordial diaphoretic medicine. He very gradually recovered his strength, and the muscles of the neck continued stiff for some considerable time.

The mother of this boy was seized with pain in the top of the head and down her back, and in both legs and arms. She also had a stiff neck, and the muscles were very rigid. She completely lost her appetite, and was utterly prostrate. Her chief pain was at the nape of the neck. She attributed her illness to exposure to the night air. Leeches applied to the neck gave her great relief. She took no food. Brandy and milk was the only nourishment she had the least relish for. Her countenance betrayed her illness. She looked most wretchedly. There was no fever. She had most distressing noise in the ears. She took quinine, and had a liniment of camphorated oil and chloroform rubbed along the spine. For a considerable time she could take no food, and felt, as she said, utterly good for nothing. She could not take interest in anything. This state of things continued for a month, when, at last, she began to improve, and this improvement showed itself in her improved expression of countenance, which seemed, as it were, gradually to expand and to lose the pinched appearance that it hitherto had. She described, too, a thrilling sensation through her, which, she said, recalled what she felt when on the occasion of having a tooth extracted, she had inhaled chloroform, and was just recovering from its influence. She had pretty well recovered, and thought she need no longer spare herself, but a return of the stiffness of her neck told a different story.

We consider it a fact worthy of recording that at the time we were attending this lady, affected as we have above described, amongst rabbits which her sons had three died, each a month old, and each in the same way; its limbs seemed to fail it, and it fell on its side, then worked in convulsions, and died. Two hens also fell lifeless from their roost. I begged of her to let me have the first rabbit that died that I might have it examined, in order, if

possible, to ascertain the cause of death. One soon died exactly in the same way that the others did; it seemed to fall down, as it were, paralysed, then worked in convulsions, and so expired. I sent it to Dr. Connor, Curator of the University Museum, and begged him to examine it for me, not communicating to him any particulars relative to the animal's death; the only unusual appearance he observed was a very remarkable congestion of the vessels at the base of the brain. I sent him two more that had died exactly in the same way. In one there was the same congestion of the vessels at the base of the brain, and in the other there was a vascularity of the membranes of the spinal marrow, indicating inflammation.

Out of eleven rabbits nine died exactly in the same way.

At the time of the occurrence of these cases, a very remarkable case of tetanus occurred in a young lady in Dublin, and excited a good deal of interest. The cause assigned was that she had fallen asleep opposite an open window. There was no wound that could account for it. The disease terminated fatally in a week. I did not learn more exact details of the case, but what I did hear suggested to me the probability that its pathological character was the same as that of those cases that had come under my observation, but in a more aggravated form; and what especially made me entertain this idea was that many of the individuals under my care complained of stiffness of the jaws almost amounting to trismus.

What, we would ask, is the pathology of these cases? Or am I correct in the designation I have applied to them? Would the term cerebro-myelitis have been more strictly applicable? Two views suggest themselves to us. Either the morbid impression may have been first made upon the heart, and have weakened it, and in consequence of this weakened state of the organ, its propelling power was lost to the capillaries of the spine, and they thus became congested; and no doubt the effects of the remedies employed would give some countenance to this view.; for the immediate relief that followed the application of leeches to the spine was most striking, and the beneficial effects of tonics and stimulants were equally remarkable. The other view is, of course, that the morbid impression was first made on the nervous system, and exercised a debilitating effect on all the parts under the influence of this system, that it was some malarious influence acting on the system. This view would seem to afford a more complete and satisfactory explanation of the phenomena that presented themselves,

viz., the pains, and the helplessness and loss of power of motion, and the overwhelming feeling of fatigue and lassitude of which most of the individuals complained. Nor did the nerves supplying the muscles, the instruments of motion, appear to be the only ones affected, but the ganglionic system of nerves also.

We should be disposed to adopt the latter view, while we confess how difficult it is to assign its proper place to a morbid complication involving the nervous and vascular systems—these two being so intimately connected and so constantly implicating each other. While we profess our preference for the latter view, we should say to others—*Utrum horum mavis, accipe.*

ART. XIV.—*Syphilitic Paralysis and its Treatment.* By WILLIAM MOORE, M.D., Dub.; M.R.I.A.; Fellow of the King and Queen's College of Physicians; Senior Physician to Mercer's Hospital; Physician in Ordinary to Sir P. Dun's Hospital, and to the Institution for Diseases of Children; Lecturer on Practice of Medicine, and on Clinical Medicine, &c.

Syphilis, Followed by Secondary Symptoms—Paraplegia—Hemiplegia—Epileptiform Convulsions, with and without Loss of Consciousness—Progressive Motor Ataxy.

The subject of nervous disorder supervening on syphilis has been treated of by various authors, among whom may be mentioned Drs. Graves, Todd, Brown-Séquard, Reade, Duncan, Hutchinson, Bryant, and others; but as these affections are daily receiving more attention the details of the following cases may not prove wholly uninteresting:—

CASE I.—*Syphilis, Followed by Sore Throat, Rheumatism, Cervico-dorsal Neuralgia, Paraplegia.*—Michael R., aged twenty-six, was admitted into Mercer's Hospital on the 6th of October, 1864.

His family history is as follows:—He had four sisters and one brother, who all died young; his mother is also dead, but his father is alive, and is a strong, healthy man.

This patient had been a waiter for the last twelve years, during which time he was very much addicted to drink, kept late hours, and was exposed to cold. About six years ago he contracted

syphilis, for which he took mercury freely; he afterwards had sore throat and syphilitic rheumatism—in short, from his first being infected his health has never been good. About three months before his admission into Mercer's Hospital he was seized with a severe pain in the nape of the neck and between his shoulder blades; for this he underwent some treatment which gave him temporary relief, but the pain returned, accompanied with shiverings. He now took Turkish baths, but obtained no relief. On the night of the 27th September he was seized with severe cramp in his right leg, and on the following day was unable to move it, and a sense of numbness extended over both legs.

On admission into hospital his appearance was anemic, and he complained of pain and weakness in the lumbar region, also of a pain and sense of tightness round the epigastrium. He had great difficulty in passing urine (which, however, had an acid reaction), and he was unable to turn himself in bed without being assisted. His appetite was fair, and he slept well.

He got five-grains doses of iodide of potassium, with one-sixteenth of a grain of strychnine, three times a day; electricity was applied along the course of the spine; at the end of a month he was able to run about the ward; and after ten days more he left the hospital, feeling able, as he said himself, "to attend a table of twenty."

Case II.—*Syphilis, Followed by Secondary Symptoms—Iritis, Epileptiform Convulsions, with Loss of Consciousness, Hemiplegia.*[a]—John D., aged forty, but looking ten years older, was admitted into Mercer's Hospital on the 7th November last. His history is as follows:—By occupation a gardener; he was a good deal exposed to wet; but in addition he had been in the habit of wading up to the waist when on fishing excursions. In 1860 he contracted syphilis, for which he took mercury freely, and at the same time his habits were intemperate; in 1861 he had syphilitic iritis, when he again got mercury. Two years ago he had an epileptiform attack, and remained unconscious for forty-eight hours; and within the last year an eruption appeared on his arms, forearms, thighs, and legs, which, judging from the cicatrices, I should say was rupia. On the night of the 3rd of May last he went to bed in his usual health, but on trying to dress himself the following morning he was seized with a loud buzzing in his ears, and fell down unconscious. From this

[a] This case was reported by Mr. John Myles.

date he tried to follow his occupation till the week before his admission into the hospital, when he had another fit, on recovering from which the right side was found paralysed. None of his family were paralytic.

On examination there was complete hemiplegia of the right side of the body; there was blunting, but no absolute loss of sensibility. He had imperfection of speech, so much so as to be with difficulty understood; there was double vision and abnormal contraction of the pupils; his pulse was slow, labouring, and compressible; the urine was alkaline, and there was an involuntary passing of the dejections. On the 7th of November a blister was applied over the head from ear to ear, and he got six ounces of wine. The following day he seemed more himself, but still passed everything under him. He was now ordered five-grain doses of iodide of potassium, with syp. of bark.

On the 14th of November he was still passing involuntarily; the urine was alkaline, but he spoke more distinctly and had more command of himself generally. The left pupil was dilating, but the right remained minutely contracted. On the 20th the treatment was changed to bromide of potassium, seven grains three times a day. During the night of the 22nd he again had the "buzzing" noise in his head and pain in the cervical region, and on the 23rd he had an aborted epileptiform attack, with unconsciousness. On the 24th he was much more lively than he had been since his admission, and gave timely warning to the nurse: the bromide of potassium was increased to ten grains three times a day. On the 7th of December we went back to the iodide of potassium. He was now able to walk about the ward with the help of a stick, and in ten days more he left the hospital so far improved as to get up on a car unassisted. I may add, the right pupil remained contracted, which may have been due to the iritis he suffered from some years before.

In this case there was no evidence of the epileptiform attacks having been caused by meningeal irritation from diseased bone; they seem to have been induced by systemic causes—such as the syphilitic poison acting upon the nutrition in general, but on the brain in particular.

Case III.—*Syphilis Followed by Eruption, Headache, Hemiplegia, Epileptiform Convulsions, without Loss of Consciousness.*—Esther D., aged twenty-four, was admitted into Mercer's Hospital

in the Spring of '62. She looked anxious and careworn, though of a full sanguineous habit of body. She stated that up to the period of her marriage, six years previous, she never had an hour's sickness, but from that time up till her admission she has been continually a sufferer. Shortly after marriage she contracted syphilis from her husband; then an eruption came all over body, attended with severe pains in all her joints, and excruciating headache. These symptoms continued, with short intermissions, till after the birth of her first and only child, which was born about a year after her marriage. She then sought advice, and was freely salivated. The eruption, pains, and headache were relieved for a short interval, till after fresh exposure to cold and hardship they returned, when she was again treated with mercury. About this time she got drooping of the left eyelid and strabismus of left eye, and she gradually lost power of the entire left half of the body. On admission into Mercer's Hospital she was in a most helpless condition, having been assisted to the ward by two persons. She literally had no power of the left half of the body, but there was no loss of sensation over the left side. She complained of great oppression, lassitude, and sense of weight in the back and loins, of continual headache and giddiness, of dimness of sight in both eyes; the bowels were constipated, and the menstrual function had totally disappeared for the past three years.

She got seven grains of iodide of potassium three times a day, with five grains of compound aloetic pill at bed-time. She was dry-cupped over the lumbar region, and took a hip-bath every night; her diet was liberal, but not stimulating. At the end of a fortnight she was able to walk into the garden of the hospital unassisted. At the end of a month all the distressing symptoms which she had suffered from on admission were abated, with the exception of an occasional headache, and she was meditating her departure, when, suddenly, at eleven o'clock at night of the 25th March, she was seized with twitching and convulsive movements in her extremities. She complained of intense headache, and vomited. These symptoms continued, with brief intermissions, till the time of the morning visit, when she was found to be weak and prostrate in the extreme

Ice and refrigerating lotions were applied to her head, whilst restoratives were used to the extremities. She got wine in small quantities, often repeated, and beef-tea freely. In the course of twenty-four hours all the convulsive movements disappeared; the

iodide of potassium was resumed, she gradually improved, and on the 26th of April she left the hospital, still feeling weak, but the power of the left side of the body being completely restored.

CASE IV.—*Repeated Syphilitic Attacks, Followed by Secondary Symptoms—Rheumatism, Occasional Diplopia, Opisthotonos, Loss of Muscular Control, Progressive Motor Ataxy.*[a]

G. M., aged 48, was admitted into Mercer's Hospital on the 1st June, 1865. The following is his family history:—His father died of apoplexy, at seventy-two, and he had two sisters and one brother, who died of phthisis. In the year 1839 he contracted syphilis, for which he got mercury. About a year after, he again got primary sores, followed by a bubo, and again took mercury, and afterwards iodide of potassium. Despite of this treatment he got secondary symptoms (in the form of eruption) which disappeared under treatment. He remained in good health for about a year, when the eruption again appeared, and he then consulted the late Sir Philip Crampton, who treated him with mercurial inunction; he also took mercury, in addition, for about a month continually, but was not salivated. At this time his habits were bad, and he led a most irregular life for some months.

In 1846 he got pneumonia, from which he states that he recovered perfectly, and he continued in good health up till 1852, when he suffered from rheumatic pains over the whole body. About this time he noticed his eyesight to be first affected; when looking at an object it quivered and dazzled before him; he complained of a sensation as if something was creeping over his body, and he occasionally heard a noise as if sticks were rattling over his head and shoulders. By degrees he observed his legs begin to fail him, and he could not walk without a stick; later still, on attempting to stand up, or hold himself erect, he would have fallen backwards. He had inordinate desire for sexual intercourse, which he was able to accomplish. In this condition he continued for some years, gradually getting worse, with large abscesses forming over the gluteal region, till his admission into hospital.

On examination the following symptoms were elicited:—He complained of inability to walk or hold himself erect, owing to the tendency to fall backwards—in fact, he could only stand semi-erect, and even then only by holding steadily by some fixed point, else he

[a] This case was reported by Mr. J. Myles, Clinical Clerk.

would have been drawn backwards. He complained of a sensation as if nails were driven into his hands and arms, feet and ankles. At times he seemed to have double strabismus; at other times the eyes were natural. Often he could see an object looking straight before him distinctly with both eyes; at other times unless he looked askant he could see nothing; but under any circumstances he saw best with the left eye. At night, if he looked at the gas with one eye open, the light appeared bright red, and if he looked with the eye previously closed the gas seemed nearly colourless. He was slightly deaf on the right side, and occasionally he faltered in his talk, but he had no drowsy or slow mode of speech. His bowels were costive: he had desire for frequent micturition, still the urine was acid and free from albumen; there was decided anesthesia over the soles of both feet. He was ordered to take fifteen grains of bromide of potassium three times a day, and he was rubbed over the course of the spine with a liniment composed of camphor-liniment and chloroform.

On June the 20th the following was his condition:—He was able to stand, but still had the tendency to opisthotonos. Before attempting to walk (which he did with the aid of two sticks) he rested on a chair, then started slowly and apparently with trepidation. However, his pace soon seemed to increase as if in spite of himself, and his legs were thrust forward in various directions, the feet coming down with a flap (usually with the heels first), and he extended his arms in a circuitous manner; at times he would lose his balance if not watched. He seemed to have power to move his extremities, but not to control the movements, and in this respect differed from the dragging and manifest loss of muscular action of the "paraplegic." He complained less of the sensation of nails pricking him, and was generally improved. One-sixteenth of a grain of strychnine was now given, with the fifteen grains of the bromide of potassium three times a day, and electricity was applied along the spine. After a few days the strychnine was omitted, and the only variation in the treatment for the space of three months was the occasional substitution of the iodide for the bromide of potassium.

On the 1st of December last he could stand as erect as he ever did, and could walk five or six miles during the course of the day; still if you directed him to shut his eyes and place his heels together he would reel and fall backwards; he objected to stand for any length of time; his eyesight was more equable, but still failed him if he

overtaxed it in the least degree; there was no inequality of the pupils. In his walk he still showed want of control or co-ordination; his knees occasionally took various directions, and his feet inclined towards the ground with a flap, the heels first; and he remarked that the heel of the left boot was much sooner worn than that of the right.

I have been thus minute in the details of this case, inasmuch as this is a form of paralysis which at present is engaging no little attention. M. Duchenne, who has given this malady his special study, divides it into three stages:—The first, in which the patient suffers from paralysis, often temporary only, of one or other of the motor nerves of the eye; from some degree of amaurosis, usually accompanied by unequal pupils, and from erratic, boring pains. In the second stage, in addition to the above symptoms, the characteristic unsteadiness of gait begins to show itself, together with diminished sensibility to touch, and pain in the skin of the lower part of the legs, in the skin of the soles of the feet especially. The interval between the first and second stages varies from a few months to several years.

In the third stage the malady becomes more profound and general, the disorders in the involuntary, co-ordinate movements increase and extend, the anesthesia increases in the parts first affected, and extends to other parts, not of the skin only, but even to the muscles and joints.

In the case I have detailed the rheumatic pains were first complained of, then paralysis of the sixth nerve seemed to follow; but as there is no inequality of the pupils for so far, I infer the third pair are intact; then followed the incontinence of urine and satyriasis, the occurrence of the general paralysis being a question of some years later.

Bourdon (*Archiv. Gener.*, Nov., 1861) inclines to the belief that congestion of the spinal ganglia is the starting-point of this affection, the morbid changes being subsequently propagated to the posterior roots and posterior columns; and Charcot, Vulpian, and Dumesnil all found atrophy of the posterior columns and roots of the spinal cord.—Schmidt, *Jahrb.*, Vol. CXIV. and CXVI.

The diseases with which this form of paralysis is likely to be confounded are "diseases of the cerebellum," general paralysis of the insane, paralysis from lead poisoning, and chorea.

Now the question may arise, why should this case come under the denomination of syphilitic paralysis? Well, in the first place, there is nothing in the family history to point to its being

hereditary further than the vague statement that the patient's father died of apoplexy at seventy-two. On the other hand, we must give due weight to the severe "sieges" of syphilis which this patient admits he suffered, and the amount of mercury he took from time to time, his habits being at the same time most irregular. Then we have a *post hoc* argument in the improvement which was brought about by a purely anti-syphilitic line of treatment; and what still further would tend to confirm the opinion as to the syphilitic character of the affection is the fact that Duchenne enumerates *syphilis* as one of the exciting causes of this malady, the others being *cold* and *masturbation*.

In the first, second, and third cases I think the syphilitic attacks were the most rational exciting causes of the nervous disorders; at the same time I do not consider every paralytic condition should be attributed to syphilis, because the patient may have suffered from syphilis in an aggravated form; still if the individual had never suffered from nervous affections of any kind, and if the paralytic symptoms occurred without any other appreciable exciting cause—and further, if their occurrence fell within the period in which the syphilitic dyscrasia generates such affections, the *fons et origo mali* may safely be termed syphilitic.

D. A. Zambaco[a] concludes that every functional disturbance of the nervous system may be caused by syphilis, such as chorea, hysteria, epilepsy, insanity, paralysis of sensation and motion, neuralgia, and affections of the organs of sense.

Dr. Thomas Reade, in a very instructive paper on "Syphilitic Meningitis" (*Dub. Quart. Jour.*, Vol. XIII.), concludes that a single symptom of cerebral disturbance, such as some form of mental disorder, may alone indicate the organic mischief in progress: a cachetic countenance may excite suspicion of its nature; but a close investigation of the previous history of the patient's diseases and symptoms are required to affix its true character.

Zambaco holds that we must look still farther back, inasmuch as we meet with such affections in patients who have never contracted syphilis in a primary form; and hence the taint may be hereditary. Under any circumstances it is quite clear that, apart from any existing symptoms of the diathesis, and from the most attentive observation of the character of the nervous affections present, a most careful *résumé* of the family history is indispensable in arriving

[a] On Syphilitic Nervous Affections. Paris: J. B. Baillière. 1862.

at a correct conclusion as to the origin of many of these obscure cases.

With respect to the treatment employed in the above cases, it may be said to have been confined to the use of two agents, viz., bromide and iodide of potassium. Many practitioners in such cases recommend the free use of mercury.

Dr. Duncan, in his paper on Syphilitic Insanity and Epilepsy (*Dub. Quart. Jour.*, Feb., 1863), recommends us to employ mercury without hesitation in the presence of any obvious signs of venereal action, or even whenever the previous history of the patient suggests a probable ground for supposing that such a condition may exist in a latent form; and Dr. Reade, whose paper I have already referred to, says the employment of mercury in this late stage of syphilis is unattended with those formidable consequences of which we are so strongly forewarned as forbidding its use. For my own part, if such an amount of dyscrasia or evidence of broken-down constitution, so to speak, did not exist in the cases I have detailed, I should have been inclined to give mercury a trial, but then it would have been in the form of the biniodide or corrosive sublimate in bark; but where the lineaments of rupia are present, or other such signs of constitutional break-up, I think it is better to employ the iodide or bromide of potassium, or the iodide of sodium, gold, or arsenic. Without doubt, in cases of syphilitic epilepsy, I should trust to the iodide or bromide of potassium, and such is the experience of Wilkes,[a] Murchison, and others, more especially with respect to the iodide.

As regards the more special treatment of the progressive motor ataxy, Vidal (*Gaz. des Hôpit.*, 1862), Duguet (*L'Union*, 1862), Herschell (*Bull. de Therap.*, 1862), and Wunderlich (*Arch. de Heilk.*, 1863), all speak of the efficacy of the nitrate of silver in progressive spinal paralysis; but Brown-Séquard, whilst admitting its value in affections of the spinal cord, where no inflammation exists, goes on to say, that in progressive muscular ataxy the treatment must vary with the peculiar symptoms of the disease with which it may be associated. Hence in the case above mentioned, believing it to have its origin in a syphilitic taint, I adopted anti-syphilitic remedies, and for so far with the most satisfactory results.

[a] Medical Times and Gazette, Jan., 1862.

Art. XV.—*On Atmospheric Conditions Influencing the Prevalence of Typhus Fever.* By Thomas Wrigley Grimshaw, A.B.; M.B.; C.M., L.R.C.S.I.; Physician to Cork-street Fever Hospital; Lecturer on Materia Medica in Steevens' Hospital.[a]

The frequency with which typhus fever has prevailed of late, and unfortunately still prevails in Dublin, together with the well-marked character and violence of the disease, as now seen in our hospitals, affords rare opportunity for the study of this formidable disease. I do not propose to discuss the cause of typhus, whether contagion is necessary to its commencement, or whether it may arise, *de novo*, in a locality into which the seeds of contagion have not been imported, but to consider some of those causes which are favourable to its spread (and, of course, *vice versa*), and which are therefore likely to assist its origin, if it do originate, *de novo*, in a locality.

It must have been observed by all those attached to fever hospitals, and by those familiar with typhus, that at all times, and especially during an epidemic, sudden rises take place in the number of patients attacked. This is especially remarkable in hospitals, where at one time we find ourselves quickly reducing the number of occupied beds, while in the succeeding week or ten days we shall find ourselves compelled, as rapidly, if not more rapidly, to increase our hospital accommodation. On becoming attached to the Cork-street Fever Hospital, these sudden rises and falls in the rate of admissions at once attracted my attention, as worthy of inquiry as to their cause. On discussing the question with my friend and senior colleague, Dr. Kennedy, we came to the conclusion, that a sudden increase of moisture in the atmosphere, more especially when in conjunction with an increase in temperature, was always followed by sudden pressure on the hospital accommodation.

Dr. O'Brien (in his report of the House of Recovery and Fever Hospital, Cork-street, for the year ending January 4th, 1827, published in the *Transactions* of this society for 1828), when speaking of the atmospheric influences which act as exciting causes of fever, seems to hold similar opinions to those which I have just advanced—namely, that heat and exhalations from the ground

[a] Read before the Medical Association of the King and Queen's College of Physicians in Ireland, on January 17, 1866.

(which are nearly all accompanied by moisture) are the chief exciting causes of fever. When treating of the causes of fever in general, having enumerated the conditions consequent on poverty, he goes on to state "that these causes alone are insufficient for the creation of such calamities, without the aid of those we call exciting causes, the most powerful of which are—extremes of heat and cold, human contagion, and malaria, or morbific exhalations from the earth, diffused through atmosphere." Further on in the report from which I am quoting, the author gives an example of how a certain condition of temperature and humidity combining may produce fever of the ephemeral class. This case, although not one of typhus, shows that this accurate observer had noticed similar exciting causes of fever to those which have been noticed by Dr. Kennedy and myself. If the case reported had been one of typhus, perhaps Dr. O'Brien would have found that, not moisture and cold, but moisture and heat were the exciting causes of the disease. It is also to be remarked that in the table of admissions into Cork-street Hospital, given in Dr. O'Brien's report, the greatest increase in admissions did not occur until the wet, or rather, variable weather commenced—namely, after the 15th of July, 1826, on which day rain fell for the first time for four months.

This opinion (of my colleague and myself) however, was founded on frequent but not very accurately-tested observation. I at once determined accurately to test the truth of our opinion by a series of minute observations, and the chief object of this paper is to bring the result of these observations under notice.

I am aware that attempts have been made to compare increase and decrease of fever with the conditions of the atmosphere, but these observations have all, as far as I can ascertain, been confined to the taking of averages for considerable periods; for instance, we read of typhus epidemics being at their height in Winter, and therefore it is concluded that the prevalence of that form of fever is favoured by cold. Again, we read of great increase of fever in the Summer and Autumn months, and some therefore infer that warm weather assists in the spread of typhus. Other writers, for instance Dr. Murchison, in his valuable and elaborate work *On the Continued Fevers of Great Britain*, very fairly concludes from those contradictory statements, that "ordinary variations in temperature have little influence over the prevalence of typhus," and says "the hygrometric states of the atmosphere have no effect on the prevalence of typhus." He seems, however, to think that cold has, indirectly, by keeping the

poor more at home, and consequently causing overcrowding in their houses, which are usually destitute of ventilation—a powerful influence in increasing the prevalence of typhus. I do not altogether agree with Dr. Murchison in laying so much stress on overcrowding, as any other effect of poverty, hunger, or dirt, might as well be taken as *the* cause, although, perhaps, overcrowding is *one* of the more potent causes of typhus.

It is not sufficient to take averages of months and seasons in order fairly to conduct enquiries of this sort, and draw accurate conclusions, from results thus obtained. For accurate conclusions minute observations are absolutely necessary.

In order to follow up this enquiry, I took the daily number of admissions into Cork-street Hospital during 1864, from localities situated in the south side of the city, distinguishing, during the last seven months of that year, the purely typhus cases from all others. Having tabulated these numbers, I constructed from these tables diagrams representing the prevalence of zymotic disease during the whole of the year 1864, and of typhus during the latter seven months of the same year.[a]

I took the precaution of ascertaining that the rises of fever admissions in other hospitals of the city corresponded with those in Cork-street, so that the curves in the diagrams may fairly be taken to represent the variations in the prevalence of these diseases in the whole city, during the year 1864. The reasons for selecting 1864 in preference to the past year (1865) for demonstrating these observations are, that at the commencement of that year it could scarcely be said there was an epidemic in this city, while, at the end of the year the epidemic had risen almost to its greatest height. The reasons for selecting the seven months, from the end of May to the end of December, 1864, are of similar nature, with this addition, that previous to May the records were deficient, owing to changes in the hospital staff and management. But to ascertain the causes affecting the variations in the fever line of the diagrams, it was necessary first to ascertain how far back I should look for those causes, in other words, to ascertain how long ill the patients had been before admission. To do this accurately was almost impossible; but taking the patient's own statements, and making all due allowance, I found the average to be five days; and as

[a] The diagrams were exhibited to the meeting before which this paper was read. The lower continuous line of the plate is a representation of the curve of the typhus diagram mentioned above.

patients usually underrate the duration of their disease, determined that I was to look back from five to six days for the causes for which I was searching. For comparison with the diagram of the rise and fall of typhus, I constructed diagrams illustrating the rise and fall of the moisture, of the minimum, maximum, and mean temperature[a] of the atmosphere, for the seven months ending Dec. 31st, 1864.

I found that it was unnecessary to consider the direction of the wind, the height of the barometer, or the rainfall, as these were causes producing the variations in the former set of conditions mentioned.[b]

The diagrams of atmospheric conditions thus constructed I placed in juxtaposition with that representing the prevalence of fever, placing them so that each day of the fever diagram would correspond with the meteorological conditions existing five days previous—in other words, placing the effect under the supposed causes. These I have so arranged, that they can be conveniently compared with each other.[c]—See plate.

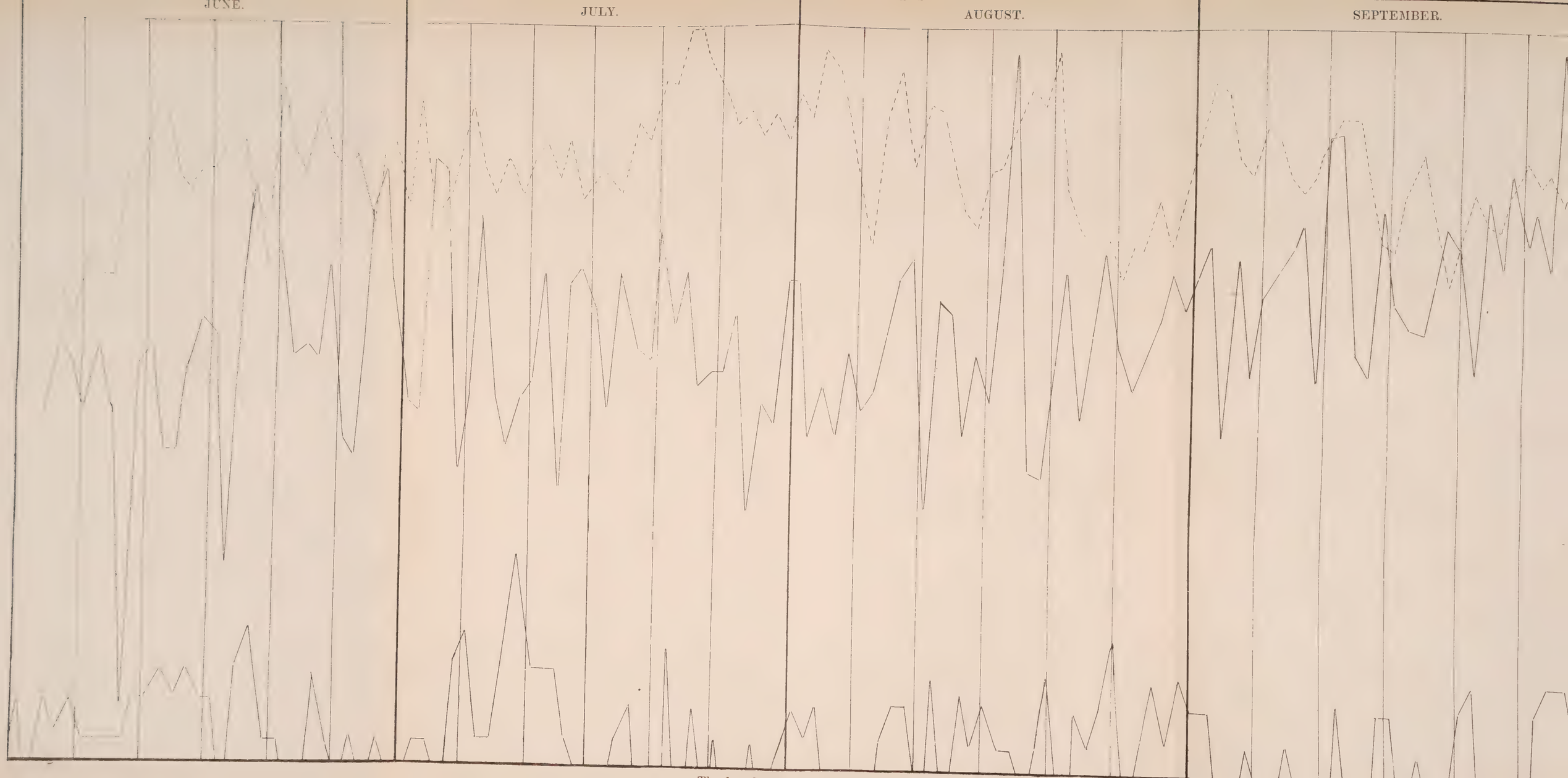
The following slight errors occur in the diagrams:—

Sundays and Mondays cause an inequality in the rate of admissions, being usually less on Sunday than on any other day during the week, and consequently more on Monday; also the month of September is incorrect, as only about half the admissions from the 3rd of that month to the 3rd of October are represented; this, however, does not influence the *relations* of the rise and fall of the line to the conditions of the atmosphere. I thought it better to leave the error as it is, mentioning it, than attempt a theoretical correction. Before carefully comparing the diagrams when finished, I felt so convinced that the moisture was the great influencing

[a] The dotted line in the plate represents the variations in mean temperature, the upper continuous line the variations in moisture. It was considered better not to place all the curves mentioned above, in the plate, as only likely to cause confusion. The vertical lines in the plate are drawn at intervals of five days.

[b] At the conclusion of the reading of this paper before the Association of the College of Physicians, Dr. Lyons suggested to the author the propriety of adding a line to his diagrams, representing the variations of the amount of ozone in the atmosphere. To do this with regard to the present series of observations was impossible, as the author had no table of ozonometric observations to refer to corresponding with the period over which his observations extended. The author hopes, at some future period, to be able fully to investigate this interesting question.

[c] A tracing combining the curves of these diagrams on one sheet, placed on a perpendicular board, with a plumb line attached, so that all could be compared at once, was exhibited at the time this paper was read.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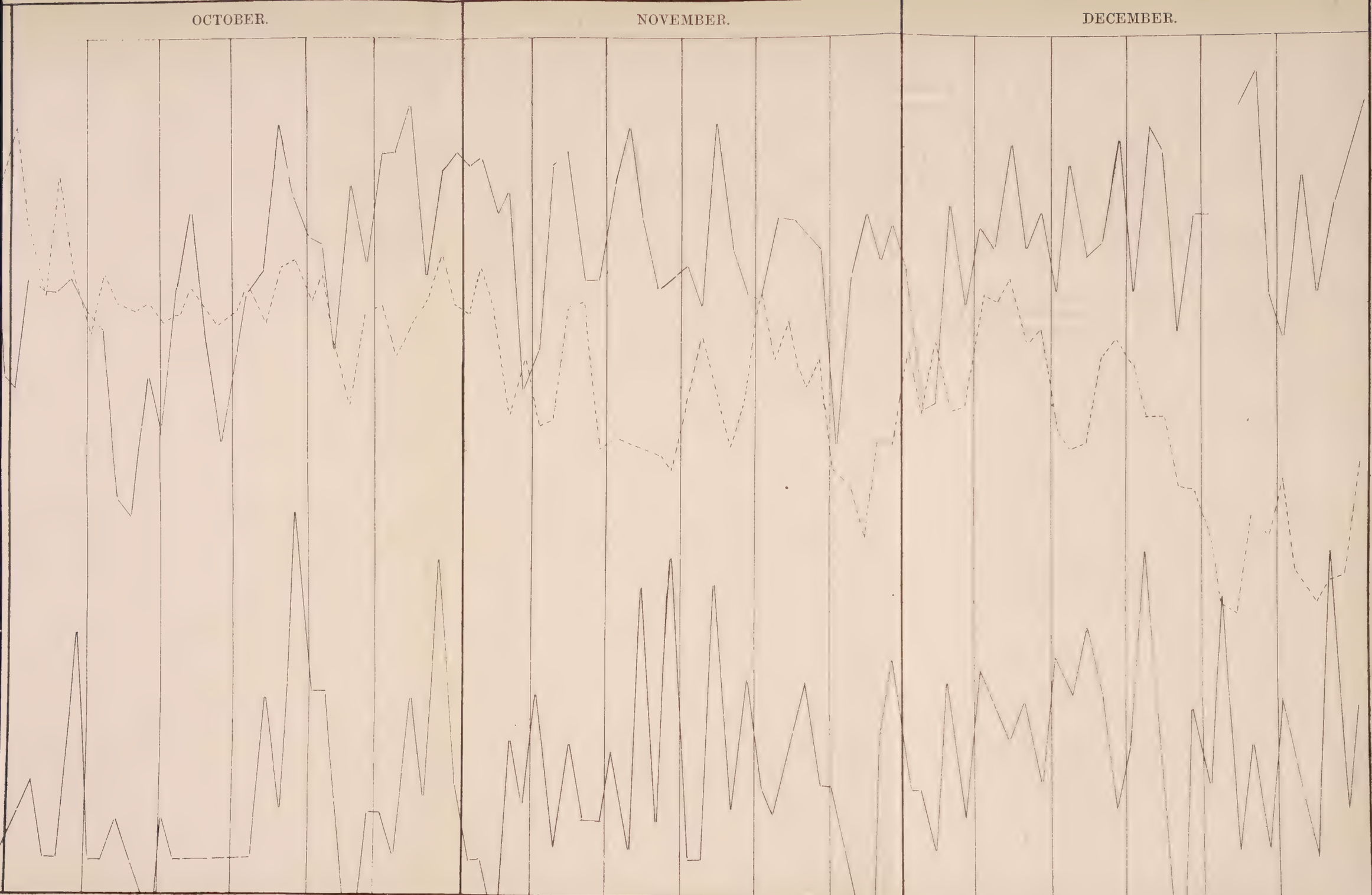

tinuous line the rise and fall of Typhus Fever, from May 29th to December 31st, 1864.

cause, that I tested my opinion by giving the fever diagram to a friend, and taking the moisture diagram myself. In taking sixteen instances at random, I stated what variations should be found by my friend in the fever diagram, and in all those instances I proved correct. What conclusions may we draw from the inspection of these diagrams?

1st. That an increase in the moisture of the atmosphere favours an increase in typhus, and *vice versa.*

2nd. That an increase of temperature favours an increase of typhus, and *vice versa.*

3rd. That the two previous conditions combined are *most* favourable to an increase in typhus, and *vice versa.*

5th. That when cold and moisture combine, the former tends to diminish the influence of the latter.

It will thus be seen that an increase of moisture, other things being equal, is the atmospheric change most favourable to an increase of typhus; but that when to this is added an increase of temperature, we have *the* most favourable atmospheric conditions for the spread of fever. It would also appear that dryness and cold (the latter particularly) have an influence contrary to the spread of fever. I do not mean to state that there are no exceptions to the rules just laid down, but the exceptions, which are few, and by no means well marked, are easily accounted for by other disturbing causes.

Since the greater portion of this paper was written, I have had *three* opportunities of verifying, in a remarkable degree, that an increase of moisture in the atmosphere, with a rise in temperature, is very favourable to an increase in fever; namely—

On December the 7th, 1865, an increase of moisture and temperature, which was observed and noted at the time, was followed by an increased number of admissions into Cork-street Hospital. On the 12th and 13th of that month the number of patients admitted was eighteen; the average for that year being six per day.

On Christmas-day, 1865 (the peculiar atmospheric conditions of which must be remembered by all), I took another note to the effect that there should, in consequence of the unusual dampness and heat of that day, be a pressure on the hospital accommodation on January 1st, 1866; this proved to be the case.

The last instance, however, is the most remarkable. On January 5th the hydrometric condition was unusually high (reported 0·9 at the Ordnance Survey Department); the temperature also was rather

high during the day, but not very remarkably so. I foretold a great rise in the hospital admissions on the 10th and 11th, which took place—21 patients being admitted on those two days, with a few on the morning of 12th, altogether more than I have known to be admitted in the same short period since my observations commenced, the rate of admission on the 11th, being the highest but one of which I have taken particular note, although I think there may be a few as high.

I have foretold a rise in the rate of hospital admissions on the 18th of this month (January), in consequence of observations taken on the 13th of January (the day following the late frost). I should like the gentlemen present, connected with fever hospitals, to observe if this forecast proves correct.[a]

I think these observations of some practical value, as all cause favouring the spread, must necessarily favour the origin of disease.

Art. XVI.—*Remarks on Diphtheria, chiefly with reference to its Contagious, Epidemic, and Fatal Character; and to its Supposed Connexion with the Cattle Plague, and with a Certain State of the Weather; founded on the Details of Several Recent Cases.* By T. W. Belcher, M.A. and M.D., Dublin; Fellow and Censor of the Royal College of Physicians in Ireland; Physician to the Dublin Dispensary for Skin Diseases; and sometime Physician to the Cork Fever Hospital.[b]

The following observations are laid before the profession, not because of their originality, for no claim is here made to anything of the kind, but solely because of the practical importance of any well ascertained facts respecting so fatal a disease as diphtheria.

The fact, often noted by observers, that the more fatal varieties of this disease are not commonly met with in hospitals renders it of greater moment that such cases as are met with in private practice should be duly recorded.

Case I.—A lad, aged about fourteen, a boarder in a large school in this city, was attacked, shortly before Christmas, 1865, with an

[a] This forecast *did* prove correct, a large increase in hospital admissions taking place on the 18th and 19th of January, 1866.

[b] Read before the Medical Society of the College of Physicians, Dublin, 21st February, 1866.

acute affection of the throat, accompanied by morbid exudation on the mucous membrane of the pharynx. He was attended in the first instance by Dr. Duke, of Harcourt-street, who subsequently called in Dr. Wharton in consultation. I understood Dr. Duke to say that the case presented the appearance of gangrenous ulceration, though, from the subsequent cases about to be detailed, he is now of opinion that it was one of diphtheria. It is quite possible that both opinions are correct; indeed, M. Trousseau asserts that in certain rare cases gangrene does affect the parts attacked with, what he terms, "Diphtherite." He observed this occurrence twice, and states that the sphacelus was confined to a small portion of the velum palati in one case, and of the skin of the arm in the other.[a] In the present case there was considerable swelling at the left side of the neck. This lad became convalescent, and was removed to Sandymount for change of air. A few days after his removal he complained of great prostration and nervous depression; head symptoms, like those of effusion of the brain in children, supervened; and despite of every care he rapidly—almost suddenly—sank, and died in what seemed to ordinary observers an unaccountable manner. There was not any *post mortem* examination of the body.

Case II.—Shortly after the occurrence of the illness in Case I.—but some time before the death in that case—a young man, aged twenty-four, a tutor in the same school, where he spent some hours daily, though he did not reside in it, became affected in like manner. His case was pronounced to be diphtheria by one of our most eminent surgeons, who attended him. He apparently recovered, but suffered from prostration of strength. Food he could, and did take plenteously; and he talked of every passing event with his usual interest. Suddenly he one day became exhausted, and rapidly sank, to the consternation of his family, who looked on him as convalescent.

Case III.—A lady, aged about thirty-six, the wife of the governor or principal of the school already referred to, and the mother of several children, had assiduously nursed Case I. through his illness in the school, where she also resided. After his death she continued for some days apparently well, until the 28th of December, 1865, when she complained of rigors; general febrile

[a] Trousseau on Diphtherite. Sydenham Society's Memoirs on Diphtheria, p. 237.

symptoms supervened; and these were again followed by every symptom of cynanche tonsillaris. The local affection was confined to the left side of the fauces.

She was attended by Dr. Duke of Harcourt-street, and on the 30th Dec., Mr. Adams was called in in consultation. These gentlemen pronounced the case to be one of unequivocal diphtheria. I saw her, with Dr. Duke, on the same evening, and I was of a like opinion. The colour and consistence of the false membrane were of the kind usually described in books; the local affection, however, as already remarked, was confined to the left side of the fauces; the left side of the neck was swollen; and I cannot better describe the tumour than by adopting the description of a case of Dr. (now Sir Dominic) Corrigan, from a clinical lecture of his, published in the *Dublin Hospital Gazette* for 15th February, 1859:—"His neck was swollen out into oval form from anterior third of clavicle to middle of neck, the oval swelling then bending inwards . . . . as it ascended, so as to leave unaffected the hollow under angle of jaw." In Dr. Corrigan's case the tumour was on both sides of the neck, but in other respects the present case corresponded exactly with his. The false membrane did not extend down the pharynx; her power of swallowing was not materially interfered with; her voice not at all affected; she complained of distress rather than pain; the pulse weak and frequent; the skin acting freely. She had been taking Dover's powders combined with nitre, wine, beef-tea, and ten drops of the tincture of the perchloride of iron every third hour, in water. At Mr. Adams' visit in the morning he had removed part of the false membrane, but in the evening the same place was covered over afresh. On the 31st of December, the tincture of iron was applied internally; the exudation had evidently extended to the nares, but not to the pharynx, so far as we could judge. After the first application of the tincture of iron she discharged her stomach, and became much prostrated. She now got plenty of brandy. The urine was copious and apparently free from lithates, but I had not an opportunity of analyzing it. The pulse became very weak and frequent; and the peculiar offensive odour from the breath was very perceptible, although I had a cold at the time. The tumour of the neck continued to increase, and I noticed over the upper lip, at the left side, a well developed patch of herpes. In the evening the disease had so far extended as to prevent her breathing through the nares. A solution had been prescribed in the afternoon for application to the false membrane; this consisted

of three drachms of hyposulphite of soda, two drachms of glycerine, and six drachms of water; to a certain extent this solution—which was recommended in the *Medical Times and Gazette* of the previous day—had proved useful. It relieved her very much, and enabled her to throw up pieces of the false membrane. It was now directed to be continued.

At 9.30, a.m. on the 1st of January, 1866, on my arrival at the house I found that she had just died. In the course of the night her brother and her nephew, two judicious medical practitioners, had come from the conntry to see her. They saw her about 5 or 6, a.m., and from one of them I learned that she then had thrown her head backwards—as a child does in croup—and seemed to breathe with difficulty. The difficulty of breathing did not, however, seem to be great; and so they went to bed. They again arrived at the house shortly before her death, and found her almost asphyxiated. Tracheotomy was not performed. There was not any examination of the body; but we concluded that death had resulted from extension of the false membrane into the larynx. I am informed that her pulse continued to beat strongly and regularly for three minutes after she had ceased to breathe. She had had scarlatina many years before; but, so far as I know, none of the other persons whose cases are here noted had suffered from that disease.

Case IV.—When the lady described in Case III. got ill, her daughter, aged about seven, was sent out of the house. Almost at once she complained of illness, was brought home again, and exhibited the unmistakable signs of diphtheria. In this case, which I attended with Dr. Duke for several days, the false membrane was chiefly on the right side of the fauces; there was a tumour of the neck of the same shape as in the mother, but on the right side, while there was a well defined patch of herpes on the left side, over the upper lip. Typhoid symptoms prevailed here; and for days the child was in the balance. More than one relapse took place; and when she became almost convalescent the tumour in the neck suppurated and caused a troublesome abscess, which is now healed; although it, for some time, left her very anemic and weak. She had the peculiar odour of the breath noted in Case III. I tested her urine, and by the ordinary tests of heat and nitric acid obtained a very large quantity of albumen, nearly half in volume as compared with the volume of the urine tested.

In this case the main principle of treatment was support by food, wine, and bark, with tincture of iron applied locally, and administered internally. She had epistaxis several times, and delirium was a prominent symptom in her case. Some weeks after her apparent convalescence she exhibited signs of great physical prostration; but now (20th February) these have been overcome to a considerable extent. She has still (21st February, 1866) to contend with the effects of disordered innervation, as evidenced by injury to the powers of speech and deglutition, and defective vision.

Case V.—When the child just referred to was removed from her father's house, she went to that of an aunt, and there slept for a night or two with her first cousin, a girl about her own age. In a few days the latter complained of sore throat and general *malaise*. I saw her; and perceiving that there was a general family panic—for the aunt had meanwhile died—I advised her removal from the house. This was done; she was well cared for; and in a few days she returned home quite well. No one else in that family got ill, so far as I know.

Case VI.—A female servant of the lady whose death I have recorded, aged about twenty-one, got ill during the illness of her mistress, and so continued for several days after the death of the latter. I attended her also at the school with Dr. Duke. Her case was one of diphtheria, with the peculiarities of those already related. The tumour was at the left side of the neck, and the patch of herpes, which was also at the left side, was large and well defined. She used a gargle of tincture of the perchloride of iron, one part to three parts of water. This proved a most useful application, and detached the false membrane in large quantities. She recovered, but still (20th February) retains some of the most striking sequelæ of the disease; she talks "through her nose," the irregular action of the pharyngeal muscles causing food to return through the nares. In fact her powers of speech and deglutition are seriously impaired; and with this she also labours under considerable defect in the power of vision.

Case VII.—Immediately on the occurrence of all this illness in the one house, the head of the family had his other children removed to Sandymount, where, in about a fortnight, his son, a

lad aged about twelve, was attacked with diphtheria. In his case the tumour of the neck, and the false membrane for the most part, were on the right side; and unlike the others, there was here no appearance of herpes. Dr. Duke continued to attend him to his convalescence. He tells me he is now quite recovered.

Case VIII.—Another child of this gentleman, a girl aged about five, contracted sore throat in the same house at Sandymount. She recovered without any symptom of diphtheria.

Case IX.—Another daughter of the same gentleman, aged eight years, got diphtheria about a fortnight after her mother's death, in the same house at Sandymount. She was ill at the same time as her brother whose case has been referred to (Case VII.) She had not any appearance of herpes, but had the tumour at both sides of the neck. Dr. Duke attended this case to the close, and he tells me that she recovered sufficiently to be able to take daily walks. One day at the close of last month she complained of great weakness, after a short attack of diarrhea. She sank, suddenly almost, and notwithstanding every effort, she died.

Case X.—A clergyman who was frequently in and out of the school-house during the occurrences already mentioned, became unwell; got sore throat and general *malaise*, and it was fully a fortnight ere I could say he was well. In that case there was a slight greyish, apparently vesicular, exudation on the fauces.

Case XI.—A gentleman who came from the neighbourhood of this city to the funeral of the lady referred to in Case III. informed me that after leaving the house where she had died he at once felt ill. I found his throat slightly congested, but after the use of a gargle he quickly recovered, without any further bad results.

Case XII.—The nurse who attended the lady that died had to leave the house with sore throat and general *malaise*. In a few days she recovered.

Nine boarders at the school to which I have referred became more or less affected with what, from the description given to me, seems to have been diphtheritic sore throat; but most of them recovered on going into the country at Christmas. Not so,

however, with one, who, I have just heard (20th February) is suffering from partial paralysis of the motor power in the upper and lower extremities, and almost total loss of vision.

Beside the above cases, Dr. Duke became unwell with sore throat, and the same happened to me. A servant of mine, who went to the house with a message from me, was similarly affected; so was a lady who called there; and lastly, the gentleman in whose family all this occurred got sore throat, but soon recovered. I myself, and most others who entered that house at this time, felt conscious of a very unpleasant smell, which immediately suggested the idea that the sewage was defective. The house, which is a very fine one, was cleaned, papered, and painted; the kitchen was investigated, and under it was found a sewer which, in the opinion of some, accounted for the smell. In that house about eighty persons had lived; and fortunately most of this illness occurred during the Christmas vacation. This smell cannot now be perceived.

On the preceding cases I may remark:—

1. That several members of the same family, with others more or less connected with them by occupation, contracted diphtheria or diseases allied thereto.

Just as when cholera is present or is approaching we find cases of diarrhea very common, and some of them not easily separated from cholera; and just as when some members of a family have scarlatina while others have sore throat only; so, to quote the words of Dr. Jenner, "One or more members of a family having exudation on to the pharyngeal mucous membrane, and others at the same time having merely inflamed throats, would be strongly presumptive evidence that the latter had diphtheria without exudation." [a]

2. It may be further observed that these cases were (at first) limited to one house. Dr. Aitken has remarked a similar limitation; [b] while the circumstances already detailed would point to the conclusion that not only was the disease infectious, but that the miasma generating it was localized.

I inquired of several physicians in this city at the time that these cases occurred, and I could not find that diphtheria was at all general; very far from it. One physician told me that he was

[a] Diphtheria. By Wm. Jenner, M.D. P. 15. Lond. 1861.
[b] Practice of Medicine, Vol. i. Edition, 1865.

then attending one bad case, in private; and the next thing I heard was that he himself was ill in diphtheria. Dr. (now Sir Dominic) Corrigan has remarked, in the lecture already adverted to, that an epidemic is mostly ushered in by dropping cases scattered here and there. Those who recollect the last fever and cholera epidemics in this country will bear him out in the truth of this remark; and they will also agree with me when I recall to their minds that in these epidemics some cases, to all human appearance, resulted from miasmata generated by bad sewage, while others had no possible connexion with any such cause. Just so with diphtheria; an epidemic is preceded by scattered cases. Any one may contract it; but those exposed to infection or to miasmata injurious to general health are specially susceptible of it. Since these cases occurred I have heard of others quite as scattered, but more frequent. From all this, taken into connexion with the history of epidemics, perhaps it is not rash to expect a general visitation of this disease. Whether it has any connexion with the peculiar state of the weather this Winter would be mere guessing; but I should like to ascertain how far previous epidemics of diphtheria were associated with weather such as we have had for some months past.

3. The tumours described in these cases are worthy of remark. In no case was the neck swollen, as in mumps, over the superior portion of the parotid gland; nor did the tumour involve the inferior angle of the jaw, as in scarlatina or cynanche. Dr. Corrigan noted this peculiarity, and evidently considered it to be a sign of value.

4. The occurrence of herpes in diphtheria I have not seen noted by any writer. Dr. Jenner indeed says that French writers describe an herpetic eruption on the mucous membrane of the pharynx, which may be mistaken for diphtheria. "It is commonly associated with herpes of the lip, and, as a rule, is much more painful than diphtheria, the pain being limited to a single spot in the pharynx."[a]

The cases to which I refer—those of unmistakable diphtheria in which there was herpes on the lip—had no appearance whatever of herpetic eruption on the mucous membrane of the pharynx. The clergyman (Case X.) who had a vesicular exudation on the fauces had *not* herpes on the lip; and, since the above cases occurred,

[a] Jenner, *op. cit.*, p. 61.

I met with another, in which a clergyman had a vesicular eruption on the pharynx preceded by partial aphonia—he had not herpes on the lip either; though in other respects his case resembled those noted by the French writers and by Dr. Jenner.

Further, a gentleman whose son was a day scholar at the school previously referred to, informed me that the latter went home with sore throat from it. This soon disappeared during the Christmas vacation, but a large patch, of what, from his description, seems to have been herpes, appeared on the forehead over the left eye.

5. The occurrence of sudden death, or what nearly amounted to it, in so many cases is worthy of note. In the cases of this kind recorded by Dr. Jenner the cause is said to be asthenia, while Dr. Aitken states that death sometimes occurs from syncope. Nearly a century ago Dr. Rutty[a] remarked of an epidemic which prevailed in Ireland in 1743, when the weather was very much like ours during the last year, that it in some points resembled Fothergill's sore throat. Of the cases then occurring he thus writes:—"Their death is generally sudden, and when least expected, the pulse being good a minute before."

In the cases above detailed apparent convalescence was plainly no guarantee of safety.

6. Like other cases recorded, there has been here partial paralysis of the muscles of deglutition as well as injury to the powers of speech, vision, and generally disordered innervation.

7. The fact of the pulse beating for three minutes after breathing had ceased is not common, but that it may occur Sir Benjamin Brodie has proved in his *Lectures on Pathology* (Lecture 66).

With reference to these cases two practical questions suggest themselves: (*a*) should the false membrane be torn off; (*b*) should tracheotomy be resorted to?

(*a*) Dr. Jenner, and most other authorities, decide against tearing off the false membrane; "to do that (says Jenner) is to commit a decided blunder." They consider that mechanical irritation of this kind aggravates the disease; while the immediate reproduction of the false membrane on the abraded surface shows the operation to be of little practical value.[b] It is quite a different matter to mop the diseased surface with a strong preparation such as tincture

[a] Chronological History of the Weather, and Seasons, and Diseases Prevailing in Dublin. Lond., 1770.

[b] In a case of putrid sore throat Fothergill notes the reproduction of the slough after mechanical separation (Works, p. 239).

of the perchloride of iron, with a view to dislodge the membrane, and so enable the medicine to have at once a local and a constitutional effect. The same end, however, will, so far as I have seen, be answered by using a strong gargle of the same tincture (see Case VI.).

(*b*) Whether tracheotomy, or, in the case of adults, laryngotomy should be resorted to, to prevent asphyxia, is a grave question.

If the patient die immediately after it, the physician who advised and the surgeon who performed it will scarcely be held blameless by the friends of the patient, who, even when life is fast ebbing, cling to hope. On the other hand, to save even *one* life out of many by such a means is so great a success that most persons will think it worth the venture, especially as successful cases now and then occur. In any case where there is blood-poisoning, and where the friends object to the risk, I do not think it would be wise to urge it; but if the friends of the patient accept the risk promptly, in a case where the operation would seem to indicate success, the opportunity should not be let slip. In Case III. of those now detailed I am not able to say what should have been done, as I did not see the patient during the approach of asphyxia; but from the character of the medical gentlemen who did see it, and their practice in other like cases, I conclude that it was not a case likely to be benefited by operation. The blood-poisoning was evident, and the case was a bad one from the outset.

I believe Bretonneau had a *primâ facie* case when he contended for the identity of what we now call diphtheria with croup; especially since Jenner and others have recorded cases in which the false membrane in diphtheria extended into the air passages. I do not, however, propose to raise this question at present; because, from the nature of the diphtheritic exudation and other symptoms, we are all now pretty well agreed on the important and essential differences between the two diseases; but inasmuch as I have observed in a very recent medical work—the newly abbreviated edition of Copland's Dictionary—that Bretonneau is considered to have erred in separating diphtheritis from gangrenous sore throat; while in the same article Fothergill and Huxham are stated to have described diphtheria, in 1748, thereby making it and putrid sore throat identical, I think it may not be out of place to endeavour to show that Bretonneau was right after all; and that Fothergill certainly, and Huxham probably, never described what we know by the term diphtheria. Almost up to the present hour

"Fothergill's sore throat" and scarlatina, or cynanche maligna, have been looked on as one disease, and classed in books accordingly. Taking Fothergill's own description, we find that the exudation, in the English epidemic of 1748, was white or ash-coloured, and then black—it sloughed away leaving gangrenous ulcers. "The redness of the skin in the face, neck, breast, and hands is another obvious and distinguishing characteristic, which in children, and young people especially, seldom fails to accompany this disorder."[a] It is scarcely necessary to say that this description will not fit diphtheria, as we see it; while it undoubtedly will fit scarlatina maligna, the cutaneous eruption of which is such as is not met with in diphtheria; although in some cases of the latter there may be slight gangrenous ulceration, as noted in Case I. Huxham noted the "gangrenous sloughs" in much the same terms as Fothergill; and had he not, in addition to the symptoms recorded by the latter, noted the formation of an exudation analogous to that in the throat on abraded surfaces on other parts of the patient's body, it could not be said that he made even an approach to describing diphtheria. It will, perhaps, be said that he described the cutaneous diphtherite of Trousseau;[b] but as he dwelt specially on the scarlatinal eruption we may safely endorse the dictum of Bretonneau, who says:—"The gangrenous sore throats observed by Huxham are also related for the most part to scarlatinal angina."[c] The exceptional cases recorded by Huxham and Fothergill may lead some to believe that diphtheria was the disease described by them, particularly as Fothergill quotes the old writers as describing his disease, while the writers of our own day quote some of these very descriptions as applicable to diphtheria; but if this lead to any logical conclusion it must be to that which now-a-days finds many advocates:—That all this class of zymotic diseases proceeds from one poison, which operates variously under varying conditions; and that these diseases are not so many separate entities "like dogs and cats," as Miss Nightingale expresses it; but that under various names we may have essentially the same thing; some important symptoms being at one time prominent, and in their turn giving place to others. That something like diphtheria was observed at this time must be allowed;[d] and

[a] Fothergill's Works, p. 225. London. 8vo. 1785.

[b] Trousseau.—Sydenham Society's Memoirs on Diphtheria.

[c] Bretonneau.—*Op. supra cit.* First Memoirs.

[d] Even in our own day Dr. Jenner writes of his own experiences:—"Diphtheritic inflammation of the pharynx sometimes complicates scarlet fever."—*Op. cit.*, p. 57.

if we consider that then, as now, the cattle plague prevailed in England, and that the weather preceding or accompanying the pestilence was unusual, and not unlike what we have lately experienced, I think the coincidences are somewhat striking. In an old pamphlet, published anonymously, in London, in 1748,[a] a physician writes to the Privy Council:—*An Essay concerning Pestilential Contagion, occasioned by the Distemper now raging among the Cattle, with a Method proposed to Prevent its Progress.* This essay is a curiosity in its way, and gives so much information about the progress of the cattle plague at that time, of its great cost to the country, and of the means proposed to arrest it, that it might well be written at the present day. In the course of the essay there is a concise summary of the various pestilences known to history; at the close of this the writer proceeds:—"I will conclude this part of my enquiry with an account of the *Pestilent Angina*, a distemper that raged at London, in the Winter of 1739. The best relation I could at that time procure was as follows:—It began with a slow fever, and soreness of the throat, and generally in about twenty-four hours there appear'd a small white speck or two upon the tonsils, which, if left to themselves, encreased in number, grew livid about the edges, and, running into one, formed a large slough all over those glands, and spread gradually over the uvula, and all the arch of the throat. In a few days a mortification began, and carried off the patient before danger was thought on; for neither the fever nor pain in the throat were violent all this while." This description, in which no mention of the scarlatinal eruption occurs, is obviously nearer to diphtheria than that of Fothergill. That it had a certain relationship to Fothergill's sore throat may be inferred from the fact, that the anonymous physician, just quoted, proceeds to give the descriptions of Aretæus, and also of Severinus, a Neapolitan of fame, in the seventeenth century, as applicable to *his* delineation. Fothergill quotes these identical authorities with an analogous view; and so do most of our modern writers on diphtheria. The anonymous writer, whom I have already quoted, proceeds to affirm that:—"The affinity is certainly very great between the pestilential sore throat and the present disease of the cattle;" and proceeds to give extracts from authorities in support of his view. Were this a paper on the cattle plague these extracts might be here given, but it may suffice to

[a] This pamphlet may be seen in the Library of the College of Physicians, where I became acquainted with it through my official connexion with that Library.

note the important fact, that, previous to or during an epidemic, domestic animals have suffered from analogous diseases. Dr. Greenhow has fully dwelt on this in his treatise on diphtheria, pp. 118, &c.; and from the early periods of Roman History no historical statement is better established. Fothergill (writing in 1748) states that the angina maligna showed itself most frequently in Autumn, and the beginning of Winter, *i.e.*, from September to December inclusive. He also "Remarked that the Summers of 1747 and 1748 were dry, with some days, in each, uncommonly hot, for this climate—the mercury in Fahrenheit's thermometer rising in the shade, and within doors, one day to 78, and during several to 75 and 76. The Autumns of the same years were as unusually temperate and warm; the wind continuing longer in the southerly points than has often been known at this season." I believe this description will answer pretty well to that of our last Summer and Autumn; and from all these circumstances I am led to expect that diphtheria, and diseases allied to it, will very shortly become epidemic in this country.

Dr. Jenner has noted the *constitutional* nature of this disease; its tendency to attack blood relations though separated by long distances from each other.

Since writing the former part of this paper I have had further information which not only illustrates the contagious nature of this disease, but also, to some extent, confirms the doctrine of Jenner to which I have just referred.

I had occasion to visit a large town in the west of the County of Cork; and there I ascertained that while the lady referred to in Case III. was ill in this city, her nephew, a young man of about twenty-two years of age, contracted diphtheria at a distance of about 240 miles from her. They had no communication with each other, nor at that time was there any other case of diphtheria in the town of which I write. This young man recovered; but his sister, a child, who lived in the same house, contracted the disease, with symptoms precisely similar to those noted in Case III. When in imminent danger of asphyxia, tracheotomy was performed on her by one of the provincial practitioners, to whom reference has previously been made. It relieved her very much; quantities of the diseased membrane of a buff colour were voided through the aperture, but she died from asthenia in eighteen hours after the operation.

Further still.—A young lady, a cousin of these parties, living in the same town, but having no communication with them during

their illness, contracted diphtheria about the same time, and died of it. In her case the disease was presumably traceable to the fact that a niece of the lady in Case III. had left the house in this city on the occurrence of her aunt's illness and had gone home, taking with her a sore throat. She slept with the young lady of whose death I am now writing.

In both these instances the apparent propagators of the disease recovered, while those apparently taking it from them died. There were no other cases of diphtheria in that neighbourhood, save in a small island some miles off the coast of Schull, where there were five cases. Since the occurrence of the above deaths, epidemic sore throat seemed to have prevailed to some extent. Immediately on my going to the town of which mention is now made, I fell ill with ulcerated sore throat, plus an ash-coloured pharyngeal exudation, which a sound practitioner in that neighbourhood burned out in three days by applying to it a strong solution of nitrate of silver.

Art. XVII.—*On Bromidrosis or Fetid Perspiration.* By Arthur Wynne Foot, M.D., Fellow of the King and Queen's College of Physicians.

The term Bromidrosis (βρῶμος, a stink, especially of beasts at rut, ἱδρώς, sudor) is employed to signify that, under various conditions the secretions of the skin which in health are almost inodorous become remarkably tainted and offensive; the circumstances under which this phenomenon appears are various, and may be conveniently arranged into three groups, each worthy of a short consideration. The first group of cases are those in which the smell from the skin is the result of the introduction into the body, for the purposes of food or medicine, of some substance which may be itself inodorous; the second group of cases are those in which the smell from the skin indicates disease, existing and progressing internally, some of whose products or results are carried outwards, dissolved in the perspiration or sebaceous secretion; in the third group of cases the cause of the phenomenon is generally unknown or very obscure, which fact is one of the reasons that this form, which is not the least frequent of the three, is often long and unsuccessfully treated. The perspiration secreted in health from the surface of the body, when cleanliness is as scrupulously attended to as it ought to

be, is almost inodorous, or at least rarely appreciable by the organ of smell, as constituted in the higher mammals, so that it has long been the practice of physicians to look upon any odorous condition of this secretion as a symptom of ill health deserving their serious attention; for this reason Paulus Egineta has handed down four prescriptions for fetor of the perspiration.

Examples of the first class of cases, in which the fetor of the perspiration is dependent upon certain articles used as food or medicine, are sufficiently common; the fetor in these cases ceases when, or soon after, the cause is removed; there is no disease or illness necessarily in the case, and the worst result is that the individual becomes personally and publicly obnoxious. Aristophanes alludes to the disgusting emanations from Asiatics who consume assafetida daily. The volatile oils contained in this gum-resin, as well as those contained in the genus allium, including the garlic, onion, shallot, and leek, being absorbed by the veins, taint the exhalations and secretions of those who use them. The perspiration of the inhabitants of parts of the Arctic regions, as the Samoiedes and Esquimaux, smell strongly of whale oil; this animal oil, often not in its freshest state, forming their chief food. The leguminous odour of the sweat of Greenlanders is attributed to the quantity of peas they eat. During the administration of phosphorus, sulphur, and tellurium, the compounds of hydrogen with these substances are emitted from the skin in an unmistakable manner. That the exhalations from the lungs are not alone impregnated with these gases is proved by the fact that silver articles become blackened in the pockets of persons undergoing a course of sulphurous medicines. The results of the union, within the body, of hydrogen and tellurium, are so offensive that it is improbable that this substance, first used medicinally by Sir James Simpson, will ever be a favourite remedy. The odour of telluretted hydrogen cannot be distinguished from that of sulphuretted hydrogen. Dr. Aquilla Smith has recorded (*Dub. Quart. Jour. Med. Sci.*, Vol. XVI., p. 194) the effects he witnessed subsequent to the administration of the nitrate of tellurium in some experiments on its action, undertaken at the suggestion of Sir James Simpson. Tuesday, August 31st, 1852, at noon, one half-grain pill was administered to each of the following patients in Sir Patrick Dun's Hospital:—Christopher Connor, aged 32, under treatment for chronic pleurisy, and John Shalvey, under treatment for a venereal eruption. The two patients lay in beds about five feet apart, in a spacious and well ventilated ward. September 1st.—

A peculiar odour was perceived from Connor's breath, but none from Shalvey. A pill was administered to each patient at noon. September 2nd.—An alliaceous odour was very perceptible; two physicians noticed it immediately on entering the ward. A third pill was administered to each of the patients at noon. September 3rd.—The odour to day was very strong, and so offensive that the nurse and the other patients in the ward complained of it; the two patients also were very sensible of it, and remarked that they had a very disagreeable taste in their mouths. Shalvey, who was a sailor, said the taste was like garlic. One of the physicians, who saw the patients the day before, was so disgusted that, on entering the ward to-day, he retired immediately. The odour was very like that emitted by metallic arsenic when volatilized. Each patient having taken three pills, and the effect being so palpable, Dr. Smith did not consider it necessary to pursue the experiment. On questioning the patients minutely, he could not ascertain that any other particular effect had been produced by the pills. Shalvey said, they made him perspire more than usual. September 4th.—The smell through the ward was diminished, but the exhalations from the patients' lungs were very offensive. September 5th.—The peculiar odour less evident on entering the room. September 8th.—Connor's breath still retained the peculiar smell very strongly; Shalvey was quite free from it. The latter patient, during the experiment, did not emit so strong an odour as Connor. The increased perspiration may have eliminated the tellurium more rapidly than in the case of Connor, who did not perspire more than usual. September 16th.—Connor's breath still retains the odour, which is not perceptible when standing close over him.

In the second group of cases, an ill smell from the skin is a symptom of disease, obvious and serious, and they deserve a passing notice. A fetor is observed not unfrequently from the skin in lunatics as diagnostic as that from a variolous patient, it may be perceived on entering a room where the person has been for some hours, and especially during the night. Dr. Tuke remarks that the room smells like a mouse trap. In the fourth visitation of the sweating sickness, the smell of the perspiration is described as "odoris teterrimi." During this epidemic in the course of a short time, and in many cases at the very commencement of the disease, the "stinking sweat" broke out in streams over the whole body. In the next visitation it was described as being thick and of various colours, but in all cases of a very disagreeable odour, which

when it broke out again after any interruption to its flow, was far more offensive than at the first, so that the body dripped as it were with a foul fluid, and it seemed as if the inward parts wanted to disburthen themselves at once of their putridity by an immoderate effort (Hecker, *Epidemics of the Middle Ages*, p. 260–1.) An unpleasant and peculiar smell is given off from the skin in typhus fever after the first week, which exhalation Murchison is inclined to believe to be a chief vehicle for the transmission of the typhus poison, and with which some fever nurses are so familiar as to be able to distinguish typhus from typhoid fever by the smell alone. This smell was noticed three centuries ago by Salius Diversus; it has been frequently alluded to by subsequent writers, by whom it has been compared to the "odour of rotten straw," or to "the disagreeable affecting scent from a person labouring under the confluent small-pox," to the smell given off by deer, or by rubbing the leaves of rue between the fingers, and to the smell of mice (*Murchison on Fever*, p. 134). Whenever perspiration, sensible or insensible, occurs during the active dissolution of the morphotic constituents of the body, it bears more or less the odour of the putrescence which is going on internally, thus during electrization of a limb affected with spontaneous gangrene, a fetid perspiration has been observed over the electrified part (*Gaz. Hebdom.* Jan. 28, 1859). The extreme fetor of the sweat in the sweating sickness may also be thus explained, as in that disease decomposition was most rapid, and the cadaveric odour so intense that the bodies of its victims had to be buried in graves unusually deep (Hecker, *op. cit.*, *p.* 262, note). Dr. Donovan writing of the effects of the famine of 1846-7, among the peasantry of Skibbereen, describes the skin as exhaling a peculiar and offensive fetor, and being covered with a brownish filthy-looking coating, almost as indelible as varnish, which he was at first inclined to regard as incrusted filth, but which further experience convinced him was a secretion poured out from the exhalants on the surface of the body, (*Dub. Med. Press.* Feb. 2nd., 1848, p. 67). Murchison also remarks that other observers have noticed that during starvation the body exhales a putrid odour, not unlike that of a corpse, and that after death putrefaction is immediate and rapid. Among other examples of odorous perspiration depending upon constitutional causes, and indicating serious disease, are the sour-smelling sweats of rheumatic fever, and the amoniacal perspiration, of the face especially, in disorganization of the kidney. The fetid secretion from the skin which attends

eczema mercuriale, mucous tubercles (although the sweat of syphilitic people is said to smell sweet), and favus, is not solely due to perspiration, as the skin is in such cases more or less broken, but to the fluids formed on the surface of superficial ulcerations.

The third group of cases in which the cause of the ill odour of the perspiration is obscure or unknown are of great importance, because an individual who is affected with fetid perspiration, while suffering from no actual bodily disease, is tormented by an infirmity which, in many cases, makes him so unpleasant to society that he lives in practical banishment. In these cases the fetid secretion is more usually confined to certain regions of the body, so that the term bromidrosis localis has been given to this form of odorous perspiration. This complaint usually affects adults of fair complexion and sanguineous temperament, more usually of the female than of the male sex; it is chronic and rebellious to treatment, not necessarily accompanied by profuse secretion from the skin, nor obviously connected with any particular kind of food or special state of health, excepting sometimes with deranged menstruation. In many cases the most strict attention to cleanliness and the greatest care of the skin seem to have little influence upon it. In women it is said to be cured permanently by marriage. It was well known by the ancients; and to this complaint Paulus Egineta referred in his prescriptions "for fetid smell and sweating at the armpits" (Book III., Sec. XXXVI.). The Greek physicians considered that an indulgence in figs was one of its causes; Eustatius, the commentator on Homer, makes mention of two sophists, called Anchimolus and Mochus, who lived solely upon figs for food, and water for drink; he adds that their perspiration was so fetid that when in the bath everybody shunned them (*Ad Iliad*, XIII., 6). The parts of the body generally the seats of bromidrosis localis are the axillæ, the pubes, perineum, and feet; it may occur about the feet alone and not elsewhere, and though nearly always partial may affect the entire cutaneous surface. The parts usually affected with fetid perspiration have some conditions in common which throw light upon its pathology and indicate treatment; they are parts in which the sudoriparous glands are numerous and large, where the temperature is continually high, where the secretion of the skin is confined and evaporation prevented, and, with the exception of the feet, where the presence of hair follicles involves the presence of sebaceous glands. There are reasons for believing that the sebaceous glands play an important

part in the production of this smell, one of which is that where they are absent, as on the palm of the hand, fetid perspiration is not observed, notwithstanding the common occurrence of profuse sweatings from that part of the body, upon which, according to Krause, the sweat glands are more numerous than anywhere else. His calculations give from 400 to 500 glands on a square inch of the skin upon the dorsal surface of the trunk, on the cheeks, and the first two segments of the lower extremities; 924 to 1,090 on the anterior aspect of the trunk and neck, on the forehead, the fore-arm, and on the back of the hand and foot; 2,685 on the sole of the foot, and on the palm of the hand 2,736 (Kölliker, *Man. Micros. Anat.*, p. 125). Another cause for the freedom of the hands from this complaint is that evaporation from them is unimpeded. The confinement of the secretion of the skin by boots, shoes, and other coverings for the feet is the real cause of its ill odour in that quarter. Hebra remarks that persons who go barefoot or wear but a light covering for the feet, and change it often, will have little trouble from "fetid foot-sweat," and that hence this seldom occurs in the female sex, although the perspiration is more copious in women. The odour in bromidrosis localis seems frequently due to a local decomposition of the perspiration, or of the united secretions of the sudoriparous and sebaceous glands on the surface of the body, the predisposing cause of the decomposition being some primarily unhealthy state of the blood, and the actually exciting cause retention of secretion in places where the temperature is high. Those who regard the sebaceous glands and their secretion as most at fault explain the immunity of the hands from fetid perspiration by the total absence of these glands in those parts, whereas they are large and numerous in the axilla and about the pubes and perineum, where developed contemporaneously with the hair follicles, they are constantly secreting a colourless fat, semi-fluid at the temperature of the body, designed to prevent chafing and maceration of the skin in those parts of the body. While the ordinary sweat glands contain a clear transparent fluid, without structural constituents, those of the axilla, whose tubes also possess muscular walls, are rich with cells containing fat and protein, so different from the contents of the others that Kölliker is inclined to exclude the axillary sweat glands from the class of sudoriparous glands, and to regard their secretion as peculiar (*op. cit.*, p. 127). Horner also considers these large axillary glands to be specially concerned in the elimination of the odoriferous secretion of that

region, and is confirmed in his opinion by finding them of remarkable size in this situation in the negro. It has frequently been observed that light-haired persons of either sex, but especially women, are more subject to fetid perspiration than those of a darker type; and in connexion with this circumstance the remark of Hecker is interesting: that those nations were visited by the sweating fever which are characterized by a fair skin, blue eyes, and light hair—the marks of the German race; he considered himself entitled to assume that these peculiarities in the structure of the bodies of the northern nations rendered them susceptible of the disease of which he was the historian, that they caused a proneness to fluxes of all kinds, and made sweating diseases endemic in the north of Europe, while the dark-haired southern nations, and the blacks in topical climates, remain, under similar circumstances, more free from them. Yet there are dark-haired persons who suffer from fetid perspiration almost to the same degree as do the light-haired; and the axillary odour of the negro is strong and distinctive.

The treatment of fetid perspiration, when not due to the use of food or medicines, known to affect the secretion of the skin in this peculiar manner, should be both internal and external. The sudden arrest of the secretion of the skin, which is sometimes the consequence of violent measures to check bromidrosis, has been followed by neuralgia and other constitutional disturbances, whence the complaint was formerly regarded as an endeavour on the part of nature to eliminate, per cutem, a materia peccans with whose excretion no therapeutical interference was allowable. Like fetor of the breath, it can sometimes be traced to derangement of the stomach and digestion, which having been remedied, perspiration will return to its natural condition, but the causes being generally subtle and various the treatment will differ with each individual case. Among the remedies most successful is arsenic, highly spoken of by Milton, who also recommends that, in the oldest cases, the hair of the arm-pits should be cut short or pulled out, and these parts, with the folds of the groins and the feet, to be washed every day with soap and hot water, and then dusted, after being dried, with rice powder; if any smell remains after this, the free use of chloride of zinc or permanganate of potash in lotion should be resorted to. Small pads stuffed with animal charcoal and secured in the arm-pit absorb and deodorize the secretions of this region; and arrangements of the inner clothing, adapted to the exigencies

of the case, should be made with the view of facilitating evaporation, as the more confined the perspiration the more concentrated and powerful is the odour. M. Stanislaus Martin (*Bull. de Therap.*, T. 65, p. 143) has contrived a mode of applying charcoal as a local deodorizer in fetid perspiration of the feet. A paste composed of forty parts of powdered charcoal, forty of water, and fifteen of gum should be thickly spread over a peice of filtering paper, flannel, or felt, stretched over a board or pasteboard; the paste is then covered over with another piece of paper, which is to be smoothed with the hand so as to remove all asperities; the whole is submitted to compression during an hour, after which the water is to be allowed to evaporate; when quite dry the sole may be cut out of the required size; the soles, being inexpensive, can be changed once or twice a day if necessary. Gaffard, of Aurillac, recommends the use of a lotion composed of fifteen grains of red oxide of lead and seven and one-half drachms of solution of acetate of lead; his directions are, to pound the oxide of lead in a porcelain mortar, add the acetate by degrees, and keep in a phial, and to shake the bottle whenever the remedy is used. In most cases it is sufficient to apply a few drops once a week to the parts affected. It will be seen that in three of the prescriptions for this complaint, given by Paulus Egineta, preparations of lead were used:—℞. 1. Of liquid alum, two parts; of myrrh, one part—dissolve in wine, and use. ℞. 2. Plunge heated molybdena (oxide of lead) into fragrant wine, triturate with the wine, adding a little myrrh until it becomes of the thickness of the sordes in baths; then use. ℞. 3. Of litharge, dr. xv.; of myrrh, dr. iii.; of ammom (cardamoms), dr. i.—mix with wine. ℞. 4. Of liquid alum, dr. viii.; of ammom, of myrrh, and of spikenard—of each, dr. iv.; triturate with wine, and use. The commentator remarks: all the authorities concur in recommending for the cure of this complaint a combination of astringents and aromatics; they, therefore, direct us to mix alum with storax, myrrh, and the like (Paul. Eg., Book 3, Sect., XXXVI.). To render the perspiration fragrant was one of the toilet duties of the upper classes in Greece; it was generally done by the use of an ointment, in which were mixed the leaves of the cypress pounded dry, and the bark of the pine. That the breath also might be very agreeable, Paulus Egineta says:—"One ought also to remember, in the morning, immediately after being dressed, to taste a small quantity of cassia or savin."

Art. XVIII.—*Cases Illustrative of the Usefulness of the Laryngoscope in the Diagnosis and Treatment of Disease.* By Robert M'Donnell, M.D.; F.R.S.; Surgeon to Jervis-street Hospital, and Lecturer on Surgery in the Carmichael School of Medicine.

The following cases will be read with some interest by those acquainted with the use of the laryngoscope; they will also serve to show, to those who are not, that this instrument is one of real value, not only in the diagnosis but in the treatment of laryngeal disease. Although no doubt there are now a good many persons who have taken the pains to make themselves familiar with the manipulation of the laryngoscope, there are still many who look upon its usefulness as at least dubious. A friend lately told me, that on asking a distinguished Parisian physician his opinion of it, he replied, with a shrug of his shoulders, that he considered it to be a very ingenious, scientific toy. This opinion will best be combated by detailing some cases which speak for themselves. Contrivances like the laryngoscope, endoscope, &c., which tend to advance our knowledge of early pathological change, to render diagnosis more certain and treatment more successful, are indeed ingenious and scientific, but deserve some other appellation than that of a toy. The cases which I now briefly detail will, I hope, be of use in removing any such impression:—

Case I.—*Peculiar Contracted State of the Glottis.*—Mary M., thirty-four years of age, was admitted to Jervis-street Hospital on 14th of October, 1864. She had been brought under my notice by Dr. Byrne, who had met with her among the patients at the Canal-street Dispensary.

The day before her admission to hospital Dr. Byrne examined the condition of the larynx along with me.

The patient suffered from great difficulty of breathing. This she said had come on gradually during the previous two months, but had not been distressing until within the last five days. Her voice was weak but distinct, and although feeble yet not of a whispering character. One was at once struck by the disproportion which existed between the symptoms. The dyspnea was extreme, while the aphonia was comparatively slight. Upon looking into the throat the traces of former ulcerations were visible on the tonsils and soft palate.

Upon inquiry she stated that five years before she had married a soldier, who had infected her with syphilis and deserted her. She had been treated with mercury. She had on the limbs remains of rupia; one ulcer of this kind on the leg was still in existence.

I confess that in this case I expected that a laryngoscopic examination would reveal syphilitic ulceration about the rima, but there was nothing of the kind. The rima glottidis was seen to be contracted in a most peculiar way. The superior vocal cords were drawn together so as nearly to obscure the true cords, the edges of which only were visible, at least in front. The true cords were neither reddened nor thickened. They were, in fact, so far as they could be seen, quite natural in appearance; they were rigidly fixed in apposition with each other almost in their entire length; posteriorly, near the arytenoid cartilages, there was a small triangular opening (the base towards the cartilages) not bigger than a quarter of an inch of the point of a pen. In attempting to draw in full respiration the rima did not seem to open beyond this extent; in producing vocal sounds this opening was closed, and the air passed between the vocal cords as usual.

In this case there was no ulceration; the inferior vocal cords were healthy, but some infiltration of the surrounding structures had contracted the rima glottidis, and impeded the action of the muscles moving the arytenoid cartilages. The history of the case pointed to a syphilitic origin for this affection.

The patient was ordered Plummer's pill, gr. v., every night; iodide of potassium and two small blisters, one on each side of the thyroid cartilage.

The noise which this patient produced by her breathing while asleep was indeed alarming, and so loud that it prevented the other patients in the ward from sleeping. She had no difficulty in swallowing, and, in spite of the dyspnea, bore with great ease laryngoscopic examination.

Before many days she was much better. The urgency of her symptoms had passed in a week, and she left hospital still continuing the same treatment.

Case II.—*In which one of the Arytenoid Cartilages having become Necrosed, was Removed.*—Patrick Kerr, a man about fifty years of age, had been under my observation for some years. He had first consulted, in 1860, on account of his suffering from fistula

in ano. He had subsequently several severe attacks of hemoptysis, and pulmonary phthisis followed.

In the Autumn of 1864 he lost his voice; and when I at that time examined him with the laryngoscope I could see an ulceration which, having commenced within the larynx, extended upwards over the left vocal cord.

From time to time I examined this patient, who became very tolerant of the instrument.

On the 17th of March, 1865, he came to me in extreme distress; he seemed to be dying, so great was the difficulty of breathing, and it was with some trouble that I could understand what he said. He told me that during the night before, something had, while he was coughing, got into his windpipe; he could feel it pricking him when he attempted to swallow, and he at first thought that it was a loose tooth which had fallen out, and become lodged there.

When I examined him with the laryngoscope (which, owing to the urgency of his symptoms, could not have been done, I fancy, if the patient had not been already accustomed to the use of the instrument) I saw a yellowish body about the size of a small horse bean bobbing up and down in the rima. I had the great good fortune to catch it in the laryngeal forceps at the first attempt, and I removed it, giving the patient instantaneous and complete relief. The body which was thus removed, and which I exhibited at a late meeting of the Pathological Society, proved to be the ossified remains of one of the arytenoid cartilages which had become necrosed; it had been still retained in its place by some shreds of ligament attached to its base; and had it not been removed in the manner described would probably have caused the patient's death.

Four months later (July 29th) this patient died, but I was not able to obtain permission to examine the larynx.

It is worthy of remark, however, that his voice, although not capable of modulation, and of a whispering character, was very distinctly audible and easily understood, even after the loss of the arytenoid cartilage.

Case III.—*Mucous Cyst near the base of the Epiglottis.*—James Connors, a young man twenty-four years of age, was admitted to the Hospital of the Mountjoy Convict Prison on January 19th, 1866. He had on the lower lip, at the right side, a cyst about the size of a cherry, which caused the lip to project, although the tumour itself was prominent on the mucous surface of the lip. This

cyst had, he said, been opened once before by a surgeon, but had returned, and he was admitted to hospital in the hope that by the introduction of a seton it might be permanently cured.

While in hospital he drew attention to something which he said he could feel with his finger in his throat, and which for the last couple of months had caused some difficulty in swallowing solids.

Nothing could be seen on simply looking into the mouth, but with the finger I could feel a soft, elastic swelling at one side of the epiglottis. The patient had no hoarseness or loss of voice, and could swallow fluids without difficulty.

With the aid of the laryngoscope I could easily see the tumour which the finger had felt. It was apparently a mucous cyst, much resembling that on the lower lip, about the size of a filbert, and close to the left side of the epiglottis—at the anterior part of aryteno-epiglottidean fold. There was no œdema of the surrounding parts. The vocal cords were healthy.

Having passed a small seton through the cyst on the lip, I, some days later, opened the other cyst with a curved bistoury, guarded to near the point with sticking plaster. The fluid which escaped was as clear as saliva, and about the same consistence. As he spat it out it was, of course, mixed with a little blood, but there was no pus present.

Having previously tipped the end of the stilet of a catheter with nitrate of silver, melted in a spirit lamp, and curved it suitably, with the help of the mirror, I introduced the caustic into the opening already made in the cyst, so as to touch the inside of it freely.

It was well in a few days, and although still in hospital (being employed as wardsman), the patient is now quite free from inconvenience in swallowing.

This case somewhat resembles one reported in the *Medico-Chirurgical Transactions* (Vol. 47, p. 7), by Mr. A. E. Durham, of Guy's Hospital, In Mr. Durham's case, however, the tumour had attained a larger size, the surrounding parts were engaged, and life was threatened.

I may mention that during last Autumn, while in the country, I was asked to see a poor woman's child, which was apparently dying from closure of the glottis.

The sounds of inspiration were like those of a child in an advanced stage of croup, and the mother said that the child had been gradually getting worse for a month. Not having a laryngoscope I made an examination with my finger, and in so doing

burst, by accident, a tumour, from which the child coughed up some muco-purulent matter, and recovered as if by magic.

Had the laryngoscope been used in this case, and a diagnosis accurately and scientifically arrived at, skill would have obtained the credit of what must be attributed to mere chance and luck.

Case IV.—*Ulceration between the Cornua of the Arytenoid Cartilages.*—Jane Cullen, twenty-nine years of age, was admitted to Jervis-street Hospital on the 5th January, 1864.

She was a very healthy-looking young woman, had been a servant, but had been obliged to leave her situation on account of having lost her voice.

A month before she had caught a cold, accompanied with severe cough and hoarseness.

The loss of voice was considerable; she could speak only in a whisper.

On examination, a small ulcerated spot was seen on the mucous membrane between the arytenoid cartilages, a similar spot of ulceration, the size of a split pea, existed on the side of the epiglottis.

A curved wire (the stilet of a catheter answers well), with a bead of nitrate of silver on the end of it, is very suitable for touching such ulcers. This was accordingly done. Violent spasm for some moments followed the application to the first-mentioned ulceration. The patient was given iodide of potassium in bark, and made to inhale a strong solution of sulphate of zinc, reduced to spray by a fluid pulverizer.

She left hospital, cured, her voice being quite restored, and the ulcerations healed, on January 17th.

Art. XIX.—*Climate of the Swiss Alps and of the Peruvian Andes Compared.* By Archibald Smith, M.D.

On the main points enumerated by Dr. Hermann Weber, in his "Notes on the Climate of the Swiss Alps," in a meterological, physiological, and pathological aspect, I shall offer a few brief observations in reference to the corresponding influences of elevation on the Peruvian Andes.[a]

[a] The author may here be allowed to say, that his observations on Dr. Weber's Notes on the Climate of the Swiss Alps, which appeared in this Journal February, 1864, were written and communicated to that gentleman without any view to publication, and it is only at his kind suggestion they are now published.

1. Says Dr. Weber:—"The temperatnre is lower; it decreases in proportion to the increasing elevation at the average rate of 1° centigrade, 1.8° F. for every 545 feet. The annual and monthly variations are less great on elevatad places than in plains."

Upon this proposition I have to observe, that the elevation and temperature on the Peruvian Andes do not bear to each other the uniform relation they appear to hold in Switzerland. At Lima, situated little more than 500 feet above the level of the Pacific Ocean, the mean temperature of the atmosphere, which no storm or tempest ever agitates, is approximately, 72° Faht. throughout the year; its ordinary minimum Winter temperature being about 60°, and its Summer usual maximum being about 84° in the shade.

But if we ascend to the western aspect of the Eastern Cordillera, where, at the silver mines of Cerro-Paseo, the elevation by barometrical measurement, according to Rivero, is 14,207 feet; and, according to Herndon, 13,802 feet by the boiling point of water, the mean temperature during the dry season, as observed by Rivero, is 44° Faht. by day, and 35° Faht. by night; and in the wet season I myself never observed it below 36° Faht. in-doors. In course of the months July, August, and September, the meteorological variations at Cerro-Paseo are very trifling; and the daily changes are unimportant—the weather being generally dry and sunny by day, and more or less frosty by night. So far, then, the temperature at Lima, as compared with that at Cerro-Paseo, decreases with the increasing elevation, but not in a regular scale of gradation. For example, at the elevation of Surco, about 7,000 feet up the valley of Rimac, the average temperature will be little under that of Lima; and in the inter-Cordillera valley of Huanuco, at about the same elevation—viz., on the Estate of Andaguaylla, the temperature of the atmosphere, as registered by me, in the shade of an open *veranda*, was, during three consecutive years, regularly ranging between 66° and 72° Faht. Here, then, we see that the decrease of heat does not proceed uniformly with the vertical elevation, and that the rule of one degree centigrade of decrease in temperature for every 544 feet of ascent above sea level, entirely fails on the Peruvian Andes.

2. "The atmospheric pressure decreases, or the air becomes thinner with the increasing height."

Herndon, in his work entitled *Exploration of the Valley of the Amazon*, states, that on the eastern slope of the Andes, the trade winds are so dammed up by the mountains that the atmosphere is compressed, and consequently heavier than it is farther from the mountains, though at a lower elevation The fact is worthy of remembrance in connexion with Andine climate, inasmuch as it shows that the Atlantic slope of the Peruvian Andes, thus favoured by the trade winds, is remarkable for humidity of atmosphere, and consequent exuberant vegetation; while the Pacific slope is equally remarkable for its vast extent of arid soil.

3. "The absolute amount of humidity in the air becomes probably less with the increasing elevation, but the relative amount or the degree of saturation, is, in general, greater in the lower mountainous regions."

The law, as here laid down, fails in its general application to the climate of the Peruvian Andes. From the elevation of 1,500 feet above the Pacific Ocean—to which height the sea vapours seasonally reach (especially in the months of June, July, and August)—to the elevation of about 7,000 on the western slope of the Andes, there is a zone of about 5,000 feet, of absolute aridness, on which rain never falls. Above *the rain line*, on the Pacific side, at, or about 7,000, to the line of permanent snow at or about 15,000 feet, the intervening region is partly cereal and partly pastoral. In this way, the whole coast and lower Andine hills of Peru, may be said to be rainless, as the drizzle that falls from May to October seldom forms into regular rain-drops. But, during the wet season of the Sierra—or the Alpine range, so called, above 7,000 feet—it rains there uniformly in the afternoon; and on the Cordillera it often snows heavily at night; though on the elevated pasture grounds the snow of night usually melts into floods of water and swollen rivers with the return of the morning's sun.

4. "The rapidity of evaporation is increased in the higher mountainous regions."

For full six months in every year, the irrigation and fertility of the lower valleys and coast of the western side of the Peruvian Andes, with its *quebradas*, or deep glens, depend on the rain and snow that fall on the Cordillera, and the cold, inhospitable pasture

lands generally called "puna" in Peru.[a] From this source it is that the rivers of the coast swell periodically. We may, therefore, infer that rapid evaporation on the Andine heights must be confined to the dry months, when the Cordillera sun is very hot and scorching; and, consequently, not uniform in degree, but dependent on variations of season.

On the sandy coast of North Peru, the province of Piura is so excessively dry and sunny, that the Summer temperature, in the shade, rises there to 90° Faht.; and, on a computed average, it does not rain more than once in ten years. But in Piura, both the radiation and evaporation must be more rapid along a desert shore-plain than on the cold and wet plains and *puna* regions of Junin and other table lands at the elevation of 13,000 feet, and upwards, above the sea.

5 and 6. "The motion in the atmosphere is considerably greater on the mountains than in the plains."

On the coast of Peru, strong atmospheric currents are never observed; and though earthquakes are common, a thunder storm has not been seen in the neighbourhood of Lima during the lifetime of any one now living.

7. "The air of the highest regions of Switzerland is free from marsh-malaria."

At the mines of Cerro-Paseo, I have seen intermittent fevers, though they never originate there, nor on the surrounding heights; they are imported there from either the malarial valleys of the coast, or the deep and sultry inland valleys where malaria prevails. But, wherever contracted in the first instance, "*terciana*" readily recurs in an individual whom it once attacks, at all elevations, indiscriminately, as I can attest from my own personal suffering for a protracted period, on coast and mountains, when labouring under this malady, which I had first been affected with in Lima.

8. "The sky is, in the sub-Alpine regions, more frequently dull by mist and clouds than either in the plains or the higher Alpine regions."

[a] This word is derived from "Pugna," now obsolete, according to the authority of the Dictionary of the Spanish Academy, and which means "regio frigoris, asperitate inhabitabilis."

During the wet, or rather the damp and misty season, on the lower maritime plains and valleys of Peru the sky is usually canopied with white flaky clouds; and the lower limit of the arid mountainous zone—which neither rain from the Sierra, nor vapour from the sea-board ever bedew or refresh—is usually in the wet season, on the coast, skirted by a wall of dense mist or fog. But the sky of the dry intermediatory valley, where the magney and castor oil plant naturally flourish, is generally throughout the whole year remarkably pure, bright, and serene. On the other hand, the sky of the Cordillera range is often shrouded in the dark mantle of the gathering storm, and the thunder rolls in loud peals, and among bursting clouds, from peak to peak in fearful majesty, during the rainy season of the high Alpine regions.

9. "The degree of insolation, or exposure to the rays of the sun, is greater in elevated situations."

On the Peruvian Andes, the highest degree of *insolation* is experienced in the dry and hot intermediatory *Quebradas*, or mountain chasms, with here and there patches of cultivated land, under the *rain line*, where fever and sun stroke are sometimes its consequences.

For full one-half the year, on the open *puna* and adjacent Cordillera, we have not only monthly, but daily obscurations of the sky, and frequently very rapid transitions of snow, hail, sleet, and rain with morning sunshine: this is the rainy season. In the dry season on the Sierra, however, the sun of the Cordillera is proverbially hot and penetrating, scorching the skin, particularly of the face, when exposed to its burning rays; just as the wind of the Cordillera, during the same season, is peculiarly cutting, desiccative, and irritating to the air passages of the nose and fauces.

10 and 11. "The respiratory movements become increased in frequency and depth, with increasing elevation; and the contractions of the heart become more frequent in proportion to the elevation."

After a quiet residence of a few days at the mines of Cerro-Paseo, the Cornishmen in the service of the Peruvian Mining Company, who were engaged there in 1826, generally got easily over the headache and sickness which attend the *seroche*, or Cordillera sickness; but the breathlessness, hurried action of the heart, and

sensation of throbbing and tension at the temples—symptoms which usually accompany a sharp attack of the *seroche*—were always readily induced by active exertion—such as climbing a steep shaft in a hurry, or attempting to run, and even to walk fast on the open plain. In its more severe and fatal examples, the *seroche* is attended with formidable hemorrhages, such as nasorrhagia, gastrorrhagia, and pneumorrhagia, &c.

With respect to the effect of high elevation simply, or *per se*, on the frequency of the pulse, when the body is at rest, and free from the influence of the Cordillera sickness, I may just notice, and not from memory, but on the authority of my Case-book, now before me, as written in Cerro-Paseo, in 1826, that in several instances of acute disease among the Cornish miners and mechanics, &c., the pulse is mentioned as being at first full, strong, and frequent (sometimes above 100), and then it gradually comes down to 80 in convalescence, and finally, to the natural standard (72) in cases of perfect recovery. Our usual practice at the mines, however, was to send chronic invalids and convalescents, especially in cases of rheumatism, chronic pulmonary disease, and hepatic congestion, with obstinate bilious vomiting, to our sanitarium, or great health-resort, the village of Huariaca, eight leagues below the mines, and on the road to Huanuco. But the old Spanish miners, when they found their general health impaired at these haunts of dissipation and gambling, used to resort to the mineral springs at Cono, and the suplhurous baths of Yanahuanca, in the subjacent valleys.

12. "The appetite becomes increased; the thirst is likewise, in general, augmented."

At Cerro-Paseo, the appetite of our Europeans was generally vigorous; and, in after years, when resident in Lima, I always found, in sending invalids from the coast to the mountains, that their digestion improved, provided the drinking water was good and wholesome. But I have no note or recollection that *thirst*, under the ordinary bodily conditions of health and repose, was increased at high elevations; indeed, as compared to Lima, I should say that thirst was little experienced at the mines of Cerro-Paseo, where, at one time, I myself resided for a twelvemonth.

13. "The sanguification is improved."

By removal from the debilitating climate of the capital of Peru, on the coast, to the cereal Andine valleys of equable annual climate, at the elevation of from eight and nine to eleven thousand feet above the level of the sea, the pale, anemic votary of fashionable town life is usually observed to gain flesh, and a ruddy cheek and lip, that are good outward signs of an improved sanguification.

14. "The nervous system becomes invigorated, the sleep, in general, more healthy."

I have myself sent the confirmed hypochondriac of the capital to the height of ten or eleven thousand feet on the Andes, with the most perfect success, and relief from this change of climate; and the martyr to spasmodic asthma in Lima, who can scarcely ever enjoy a perfect night's rest there, is often entirely relieved of breathlessness, and able to sleep comfortably, by changing his residence from the capital to some convenient Andine climate.

15. "The activity and energy of the muscular system became increased."

Within certain limits this proposition is true, also, on the Andes, but not absolutely. The native-born mountain Indian of Peru is a deep chested and firmly limbed little man. He is capable of very great muscular exertion, as has been proved on marches of extraordinary length and rapidity, with only toasted Indian corn for food, and the coca-leaf as a sustaining cordial. He trots, rather than walks, over ridges of the Cordillera, with perfect ease to himself. But, on the other hand, the Creole of the sea-board is not able for the pedestrian feats of the mountain Indian, on the loftier regions of the Sierra. At the elevation of nine or ten thousand feet, the native of the shores of the Pacific as well as the European, may feel his muscular energy not diminished, and perhaps increased, but as he ascends higher and approaches the snow line of the Cordillera, his muscular strength and freedom of breathing are sensibly diminished, and both fail him on any active exertion in climbing or walking, &c. Thus, the Cornish miner failed in muscular power at the mines of Paseo. He could not perform his day's work with the same ease as the native Indian, nor wield the same weight of hammer in the performance of it. But were the circumstances reversed, and the Peruvian Indian transported to

Truro, in Cornwall, with the help of all the coca in the world, he would certainly have no chance in muscular energy and power, as matched against the native Cornishman.

16. "The secretion of the skin is most likely augmented."

At high and cold elevations on the Peruvian Andes, it is found difficult to produce perspiration, medicinally, even with the aid of heavy bed-clothes. In warm and dry inland straths—such as that of the beautiful valley of Huanuco, for example—where rain only falls in passing showers, and where, for several months in every year, the night-dew is almost imperceptible, I have observed in my own person, that even during the most active exercise of running, the perspiration was so rapidly carried off the surface of the body, or absorbed by the air, that no inconvenience was felt from it. In climates of so mild and equable a character as this pointed at, it is probable that the average secretion of the skin on the Andes may be augmented.

17. "The urine appears to be not materially altered in quantity."

The climatic influence on this secretion I found to be very remarkable. Descending from the valley of Huanuco to Lima, during the dull, misty, and drizzling months of the year on the Peruvian coast, the calls of micturition were so frequent as to be most troublesome. I think the difference of temperature, in the shade, at Huanuco and in Lima at this season, might be about six degrees Faht. But in the former place, the air was dry and equable, with abundant sunshine; while on the coast the sky was dull, and the air humid, and produced a sensation of remarkable chilliness, which had the effect of throwing back the cutaneous secretion, and proportionally augmenting the secretion of urine.

ON THE PATHOLOGICAL INFLUENCES.

The type and pathology of diseases greatly vary with elevation, temperature, humidity, and aspect, &c., on the Peruvian Andes.

This subject, however, is far too extensive to be taken up in cursory notes and observations like the present. I may, nevertheless, adduce a few facts in illustration of the influence of aspect and elevation on climate, and, consequently, on physiology and pathology.

Mr. C. R. Markham, in his work entitled, *Travels in Peru and India*, in connexion with the chinchona plant, relates that the climate of Cuyo-cuyo, situated on the eastern aspect or slope of the Andes, in the Peruvian province of Caravaya, is much milder and more tropical in its vegetation than the climate of Arequipa, on the western declivity of the same mountain range; yet Cuyo-cuyo is 10,500 feet, while the city of Arequipa—which is surrounded by a rich and highly cultivated oasis in the desert—is only 7,850 feet above the Pacific. Between this city and the sea is a broad sandy desert, and behind it, towards the Cordillera side, the snow-capped "Misti," with its overhanging mantle of blue, rises to the height of 20,300 English feet. This immense mountain is treeless, and mostly denuded of vegetation, while its arid slopes are covered with volcanic ashes. Dr. Mateo Paz Soldan observes that from the end of May to the first week in August, the thermometer (Reaumur) ascends in Arequipa from 10° to 15°, and that during the remaining months of the year it goes up to 18°, as its maximum in the shade.[a] The daily transitions are sudden, and there is observed in course of the day great difference of temperature between the shady and sunny side of the same street. After sun-down the thermometer often falls 20 degrees Faht. In December, January, and February, rain falls at Arequipa, but with such marked regularity, that, according to Mateo Paz Soldan (who was himself a native of that city), it only occurs from two to five o'clock, p.m. But let us leave Arequipa, in lat. 16° 20′ S., and lon. 71° 32′ W., and cross the western Cordillera to the open sugar-growing valley of Abancay, in the department of Cusco, and in lat. 13° 37′ S., and lon. 72° 30′ W., and at the same elevation above sea-level as Arequipa, we there find a hot West-Indian climate. But, if we again leave Abancay and cross to the Atlantic side of the eastern Cordillera, we find that the relation of vegetation, heat and moisture, to elevation is totally changed from that which prevails on the western side.[b]

In cases of pure *seroche*, which always depend on an over-attenuated atmosphere, the removal of the sufferer from a higher to a lower level, as from Cerro-Paseo to Huariaca, at once removes the malady. So far, then, the connexion between vertical elevation and the Cordillera sickness is quite obvious. But in another disease, the limits of which have long been assigned to a few

[a] See Geographia del Peru. Paris, 1862.

[b] See my paper on the Greography of Diseases in the Climates of Peru. Edin. New Phil. Journal, Vol. vii., January, 1858.

thousand feet above the sea—viz., yellow fever, as seen in Jamaica and Mexico, &c.; the late epidemics in Peru have far extended these limits, and exhibited modifications of a very remarkable kind in connexion with elevation and temperature, &c. For example, when persons infected with this fever arrived in the city of Arequipa from the seaport Islay, many of them died with black vomit; but in each separate case the germ of the disease seemed to have died with the individual. It did not propagate itself or become epidemic in Arequipa, and it has been inferred that this exemption was owing to the dryness of the locality. In the still drier climate of Piura, in N. Peru, the yellow fever epidemic was exceedingly mild; and cases of black vomit were there, as in Arequipa, confined to individuals from the seaport. But, strange to say, this fever, which first showed itself on the coast, and did not, when introduced to Arequipa, spread within the immediate environs of the city, no sooner took effect beyond its suburbs, on the road to Puno and Cuzco, than it proved most mortiferous. At the poor Indian villages, Chihuata and Pocsi, five leagues to the east, and south-east, of Arequipa, in a cool climate, at the elevation of from nine to ten thousand feet above the Pacific, the fever committed great havoc, and also extended *inland* to high elevations, in the department of Cuzco, &c. It is quite true, that at such high elevations on the Andes this yellow fever did not originate; but experience proves that it preserved its vitality and individuality of type in regions where the mean temperature did not fall below fifty degrees of Faht., and at an elevation of above 11,000 feet. At still higher elevations the epidemic did not cease; but it was observed to change its type more to the form of typhus, without any marked lesion, on *post mortem* inspection, being observed in the stomach or intestines—organs which were notably affected at lower Andine levels, of from 1,800 to 10,000 feet above the sea.[a]

In the normal atmospheric constitution of Arequipa, catarrhal fever is one of the prevalent disorders of the locality, and it often merges into the inflammatory form. The *tabardillo* fever is one of the scourges of the population. It ordinarily sets in with inflammatory symptoms, and after eight days duration, assumes the low nervous form, with cerebral lesion and prostration of strength. Pleurisy, pneumonia, and dysentery (probably aggravated by the

[a] See article on Hæmagastric Pestilence, in the new and abridged edition of Dr. James Copland's Dictionary of Practical Medicine, with its references on the Yellow Fever of the Peruvian Andes.

bad river water chiefly used by the poorer inhabitants) are, also, but too common and fatal diseases.[a]

Goitre, so abundant in the warm and deep valleys of the department Curco, is not once mentioned among the diseases prevalent at Arequipa. Indeed, goitre is properly a disease of the inter-Cordillera glens and valleys of Peru; and, as for *cretinism*, I never, in a quarter of a century, met with as much as a single example of it, either on the coast or mountains of Peru. But, on strict inquiry at Lima (so late as 1859 and 1860) I learned that in the Province of Pataz,[b] and in the village of the same name—where goitre is prevalent, and the temperature warm—with a population of 1,500 souls, there are some deaf, dumb, ill-shaped, ugly, and idiotic human creatures, who may be affected with cretinism. But if cases of this kind are ever seen in Peru at the elevation of the Cordillera, or, as it has been asserted, as high as 14,100 feet above the sea, it must, I think, be a mere incident of travel, and not because the disease is indigenous at the foot of the snow-line. This I have shown more fully in an article titled "Gleanings in Peru," published in the *Edinburgh New Philosophical Journal*—New Series, 1860.

During my residence at the mines of Cerro-Paseo I never had before me but one case of apoplexy. It arose from careless exposure to insolation.

Contrary to what might at first be expected, I have known organic disease of the heart, with aneurism, quite relieved by a removal from the coast to the mountains. This case proved very oppressive, and ultimately fatal, in Lima, to a young officer who, during active military service at the elevation of ten or eleven thousand feet on the Andes, in the valleys of Tarma and Jauja, felt his breathing quite free, and was capable of taking most active exercise.

Incipient tubercular phthisis, usually attended with more or less hemoptysis, is one of the most common pulmonary affections known in Lima, and other parts of the coast of Peru. It is, besides, a disease almost certainly curable, if taken in time, by removing the coast patient so attacked, to the open inland valley of Jauja, which runs from ten to eleven thousand feet above sea-level.

[a] Chicha, or Indian corn beer, is chiefly used, instead of water, by the rural population of Arequipa.

[b] The Province of Pataz is full of deep and hot valleys with high hills between, and the goitre is there ascribed to the insalubrity of the spring water used by a section of the inhabitants.

This fact has been known and acted upon from time immemorial, by the native inhabitants and physicians; and I have myself sent patients from the capital to Jauja, in a very advanced stage of phthisis, with open ulceration and well marked caverns in the lungs, and seen them again, after a lapse of time, return to their homes free from fever, and with every appearance of the disease being entirely arrested. But in many such instances it would, after a protracted residence on the coast, again become necessary to return to the mountains to prevent the recurrence of the malady.

My space will not allow me here to enter on this subject, which I have elsewhere noticed more in detail.[a]

I may, however, say in conclusion, that at the distance of twenty-five leagues from the town of Lambayeque, on the coast, is situated the Indian village, called Cachen, on the lowest summit of the western Cordillera, as it approaches the shore of the Pacific; and the water-shed falls equally to the east and to the west from this ridge. Its climate produces wheat, potatoes, and pastures, but no tree except the elder. In the open air at midnight, and in the dry season, the thermometer of Reaumur here fell to 4°, and rose to 7° or 8° by day within doors. In the sun the rise would probably be nearly twice as high as in the shade. Now this is one of the acknowledged health resorts for phthisis and hemoptysis contracted or developed on the coast of North Peru, and cases of recovery at this place are well accredited among trustworthy natives, who have themselves experienced its advantages. But the equable climate of the strath of Huanuco, with a night and day range of temperature from 66° to 72° Faht. all the year over, is not favourable in similar cases, as I had the opportunity of testing: but at colder elevations up the sides of this Elysian Valley, I have known decided benefit accrue to the phthisical and hemoptic patient.

I am led to offer the above notices on certain Andine climates in reference to tubercular consumption, with and *without* hemoptysis, and in the white as well as in the mixed races of various shades of colour, such as are usually sent from the coast to the mountains of Peru for restoration to health, as a response to Dr. Hermann Weber's appeal to the whole medical profession for information on the influence of the Alpine climate of Switzerland, beyond the elevation of 5,000 feet above sea level, on consumption, and on the

[a] See Edin. Med. and Surg. Journal, No. 144; und also the Brit. and Foreign Med. Chir. Review, No. xxxvi., Oct. 1856.

very first stage of this disease. And though the elevation of 5,000 feet in the tropical Andine climates of Peru implies very different physical conditions, as the preceding observations sufficiently manifest, to the same height above the sea in Switzerland; yet, as regards the influence of elevation as favourable, or otherwise, to the production, or removal of, pulmonary consumption, in comparison to localities on the sea-board or lower plains, I think the general question involved in Dr. Weber's inquiry is essentially the same in its practical application on the Andes as on the Swiss Alps.

In regard to Peru, it will be proper always to bear in mind that its great sanitarium for hemoptic and phthisical patients from the capital, is the valley of Jauja, in the central department of Junin, at the distance of 120 miles E. of Lima, and beyond the western Cordillera, in lat. 11·50′ S., 75·25′ W. The breadth of this valley varies, but it may average ten or twelve miles; while its length is stated to be, little more or less, fifteen Spanish leagues, or about fifty miles. In the harvest season it is delightful to see the whole strath of this vale, to the extent of from forty to fifty square leagues of the most fertile soil, all covered, up to the base of its sloping and pastoral hills, with abundant crops of Indian corn or maize, wheat, barley, and potatoes, with intervening fields of green lucern, potatoes, and other roots. The river Jauja, which originates in the lake of Chinchaycocha, on the lofty plain of Junin, glides majestically through this valley; and on its left bank (amid numerous villages and hamlets, enlivened and beautified with trees of the most verdant foliage, scattered on both sides of the river) stand the two important towns, named Jauja and Huancayo, which are the principal health resorts for phthisical invalids from Lima. Huancayo enjoys a milder and warmer climate than Jauja, being considerably less elevated above the level of the sea than the latter. At Huancayo, 12° 0′ S. lat., and 75° 12′ W. lon.; the annual range of temperature in the shade may be taken as ranging from 8 or 9° to 14° Reamur; while at the cooler town of Jauja, with from 10 to 15,000 inhabitants, the range during one whole year has been observed not to exceed from 8 to 12 degrees of Reamur, or from 50 to 59 or 60 Faht.; with a sky always clear and sunny, and an atmosphere pure and bracing, which invites to out-door exercise and enjoyment. The harvest being home, the whole rural population rest from their agricultural labour for eight months in the year, which they give up entirely to amusement and feasting, trusting to the rain of heaven during the other four months in the year to

fertilize their land, and yield them more food than they require; so that they never think to extend their toil over the rest of the year by availing themselves of irrigation from the bountiful waters of the Jauja, as they might easily do. But there is no inducement; for the transport of produce to the capital would, in the present state of the roads, cost more than it was worth in the market. (See *Rerista de Lima*, Oct., 1859). I think this information may be useful to the European, as showing the climate and topography of the great health resort of the consumptive in Peru. It is probable that ere long the interests of commerce, and the growing facilities of fluvial navigation, will open up the inter-Cordillera regions of Peru to the whole world. Now that a new ocean steam route is about to be opened up by the United States and Brazillian Steam Company, Para, at the mouth of the river Amazon, is to be one of the ports of departure and call—whence Brazilian and Peruvian steamers will run 2,500 miles up the Amazon. Thus, the department of Junin, with its diverse geographical zones, in the provinces of Huanuco, Cerro-Paseo, and Jauja, will, in the natural course of events, become accessible from the side of the Atlantic without the difficulties of the great western Cordillera barrier to encounter: and a health resort to all the phthisical patients of the world may be easily reached, through a graduated elevation of space and climate such as the face of our globe no where else can equal, in richness of vegetable and mineral products, within the same limits. In further evidence of the importance attached to the climate of Jauja by the physicians, government, and inhabitants of Lima, and the coast of Peru in general, I shall here briefly refer to the "Estadistica General," or general statistics of the capital, as published by Dr. A. Fuentes, of Lima, in 1858. He says, on the subject:—"Jauja has always been the refuge of consumptive people, and a lengthened experience has demonstrated the favourable results of its climate." Nevertheless, he well observes, many are disappointed of the benefits they expect from Jauja, because they do not leave Lima for that district until in the last stage of pulmonary decay; or because they do not remain in Jauja the time required by nature to insure a perfect recovery; or because, after visiting this sanitarium, instead of following a methodic manner of life suited to their state of disease, they abuse the advantages of the climate by the very help of the relief they derive from it, and commit excesses which can only lead to their premature death. But the important result, from the best procurable data, notwithstanding such instances of irregularity, is,

as stated by Dr. Fuentes, that the proportion between the *cured* and the total number of patients in all stages of pulmonary consumption sent to Jauja, amounts to $79\frac{42}{83}$ per cent. And in view of so important a general result to the patients from the capital, where the Indian soldier is singularly prone to phthisis—a disease almost unknown in his native hills—the government have of late years—in 1860—initiated a military hospital for consumptive patients from the coast, and capital in particular, in the vale of Jauja, under the direction and superintendence of Dr. Jose Cobian, himself a sufferer from incipient tubercular phthisis, and recommended by the *Medical Society* of Lima to seek his restoration to health in the valley of Jauja.—See *Lima Med. Gazette*, 15th March, 1860.

The annual rate of general mortality from all diseases among the whole population of Lima, which, by census, is reckoned at 100,000, Dr. Fuentes, on strict medical investigation, estimates at 5 per cent. And, further, from a general classification of all diseases in persons of all ages and sexes, who die annually in Lima, this most intelligent and inquiring author gives us the proportion to this total, $38\frac{1}{2}$ per cent. of fever cases; $19\frac{2}{3}$ per cent. of dysenteric cases; and the proportion of deaths from tubercular phthisis, as compared to the gross amount of deaths from all other ascertained diseases, he gives at the high rate of $22\frac{22}{39}$ per cent. This being so, we must admit—even should these statistics be allowed to be as yet imperfect—that the physicians in the capital of Peru, in common with all other ranks and classes of its rapidly increasing and variously coloured population, have ample opportunity of testing the *comparative curability* of this disease on the coast and on the mountains, at the elevation of from 5,000 to 10,000 feet, and they have fully and unanimously decided in favour of the latter, provided only the patient be sent there in the early stage of the disease.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Chloroform: its Action and Administration. A Handbook. By Arthur Ernest Sansom. London: Churchill & Sons. 1865. Small 8vo, pp. 192.

Since the appearance of the masterly treatise upon chloroform, by the late lamented Dr. Snow, up to the appearance of the present work, we confess that we had been labouring under the impression that a new book upon this subject should be what our Continental neighbours would term *un embarras de richesses*—that all requisite information upon chloroform, its uses and abuses, *and more*, was contained in that exhaustive treatise, and that a new author, selecting chloroform for his theme, would find himself in the position of one having to tell a twice-told tale; yet have we lived to change that opinion, and to have to admit that this present production of Dr. Sansom's will find its appropriate place in our medical libraries. The great objection to Snow's work was its diffuseness, and its consequent bulk; but at the time he wrote it could not well be otherwise. Chloroform, as an anesthetic agent, was struggling for its very existence. Many were in the habit of using it, almost daily, who were completely in the dark as to its full potency either for good or evil; and we believe that with truth we may aver that, until Dr. Snow's work appeared, the bulk of our profession were divided in their opinions as to its merits—some blindly swearing by it, others as blindly decrying its use. That deaths occurred during its administration could not be gainsaid; but the reason why could not be adduced in any way approaching a satisfactory manner until Snow commenced the investigation in a truly philosophic spirit. To illustrate his views the introduction of numerous cases became essential; hence the increased bulk of his work. Now-a-days, however, that we have become more familiar with this agent—

thanks to the labours of Dr. Snow—it seemed that either a condensation, an epitome, as it were, of his *magnum opus*, would be an acceptable boon to the hard-worked practising surgeon of the present day; or else, and better still, an original treatise embodying all the great facts to be found in Snow's work, unencumbered of its experiments and its cases, and fully written up to our present state of knowledge upon this most important subject; and such a work, we advisedly say it, is that now before us for consideration. On some minor details, 'tis true, we might feel it our duty to differ from Dr. Sansom; but, taken as a whole, we can but express our opinion that his work is admirably suited for the busy student and practitioner—men who, of necessity, should know all the leading facts connected with this most important anesthetic agent, and yet have not time to devote to the perusal of a ponderous volume.

The most important point—that, in fact, upon which every question of safety connected with the administration of chloroform depends—is the per-centage of it, mixed with atmospheric air, that the patient can respire with impunity. The amount of this per-centage, in general terms, may be stated to have been given by different authorities at from 3 to 5 per cent.—the latter per-centage being that adopted by Snow; however, the most recent authorities seem to approve of a state of dilution of about $3\frac{1}{2}$ per cent. of chloroform mixed with atmospheric air as being most consistent with the patient's safety. Vitally mixed up, however, with this subject is another, which at first sight might not strike the operator's mind, and yet which it is all-important, especially in the but too prevalent way of administering of chloroform—poured out at haphazard on a napkin—that we should weigh well and estimate to its fullest extent—that is, the amount of chloroform vapour that the atmospheric air is able to take up and retain in solution: a question that becomes of still greater importance when it is known that the amount varies with the temperature of the surrounding atmosphere—the higher the temperature the greater being the amount of chloroform that the air is capable of absorbing. This question is so fully expressed in the tables in Dr. Snow's work that we hesitate not to reproduce them.

"The following table shows the result of experiments I made to determine the quantity of vapour of chloroform that 100 cubic inches of air will take up, and retain in solution, at various temperatures.

Temp. Fah.	Cubic Inches.
40°	7
45	8
50	9
55	11
60	14
65	19
70	24
75	29
80	36
85	44
90	55

"In the above table, the air is a constant quantity of 100 cubic inches which becomes expanded to 107, and so on; but it may be convenient to be able to view at a glance the quantity of vapour in 100 cubic inches of the saturated mixture of vapour and air, at different temperatures, and in the table which follows the figures are so arranged as to show this.

Temp. Fah.	Air	Vapour
40°	94	6
45	93	7
50	92	8
55	90	10
60	88	12
65	85	15
70	81	19
75	78	22
80	74	26
85	70	30
90	65	35

"As the effects of chloroform when inhaled depend entirely on the quantity of vapour present in the air which the patient breathes, the effect of temperature on its volatility is of great practical importance. The air, it is true, does not become fully saturated with vapour during the process of inhalation, but the effects of temperature are relatively as great. If, for instance, a person inhales chloroform from a handkerchief or an inhaler, in such a way that the air he breathes shall be half-saturated with the vapour, then supposing the temperature of the apartment, the handkerchief,

etc., to be 50°, the air he breathes will contain 4 per cent.; but if the temperature be 70°, the air will contain 9·5 per cent. of the vapour."

Now let us realize this fact, presuming that the thermometer is standing, as stated by Snow, at 50°, and that the air becomes but half saturated with chloroform, the patient inhales but four per cent. of the anesthetic, a quantity perfectly compatible with safety to life; but let the same amount of chloroform be poured out on a napkin and applied to the patient's nostrils whilst the thermometer stands at 70°, he is exposed to an atmosphere, presuming, again, that the air be but *half* saturated with chloroform, containing 9·5 per cent. of the anesthetic, a per centage far beyond even the highest estimate of what can be inhaled with safety. How seldom does this most important fact enter into the calculations of the parties who take upon themselves to administer chloroform; how slightly do they consider the practical bearing of the question whether they are administering chloroform in Winter or in Summer, in a warm or in a cold apartment; and yet upon such considerations as these hangs the life or death of the patient. Considerations such as these justify every scientific chloroformist in his objections to the napkin, folded lint, towel, and all such agents, as the medium for the inhalation of the chloroform; how frequently do we not see the chloroform recklessly poured out upon some such medium and forcibly held to the patient's mouth and nostrils, no means employed even for measuring the quantity used; and can we wonder at the occurrence occasionally of fatal accidents, or rather should we not be surprised that such are not far more frequent? At first the patient's repugnance to inhale a highly saturated atmosphere may afford him, unwittingly, some protection; but as the anesthetic progresses, even this protection is removed, and so far as the chloroformist's skill is concerned, no further safety exists for his patient than that afforded him by chance; need we wonder then that any person who has scientifically considered this question, and attempted to secure the maximum of safety during the administration of chloroform, has devised some modification of an inhaler that will secure for us this most desirable object. By the folded napkin we have no means at our disposal for regulating the due dilution with air of the chloroform vapour, whilst the ordinary method of dropping the chloroform from the mouth of the bottle containing it, is in itself open to grave objections. So much depends on the

size of the mouth of the bottle and on the operator's steadiness of hand, the slightest accidental motion even on the part of a by-stander being sufficient to cause an undue delivery of the anesthetic; all these considerations, we repeat, have resulted in an anxiety to devise some apparatus which would be free from these fertile sources of danger. In Dr. Sansom's work three of such apparatuses are described, namely, Snow's, Clover's, and one of his own invention, which, so far as we are enabled to judge of it from his description, appears to be an admirable apparatus, fully capable of discharging all the duties that can be required of it. In another portion of his work he incidentally alludes to Dr. Skinner's gauze frame work, acknowledging its merits, so far as its power of effecting a sufficient admixture of air with the chloroform vapour, but seeming to doubt its capacity for producing an atmosphere sufficiently concentrated for producing a profound narcosis, in fact, a fault the very opposite to that but too justly alleged against the use of the napkin. In his allusion to Dr. Skinner's apparatus, Dr. Sansom, we are sure perfectly unintentionally, seems scarcely to have done that gentleman justice, having omitted in his description a most important portion of the apparatus, the supply bottle, from which by no ordinary casualty can an over-dose of the chloroform be delivered. And as to the practicability of producing deep narcosis by its agency, all we have to remark is that repeatedly have we used it in even the severest surgical operations, ovariotomy, amputations, &c., and on every occasion it has more than fulfilled our most sanguine expectations.

To allow our readers to judge of our author's *style*, no unimportant element in any work, we shall extract the following passages, in which is contained much sound advice, first, as to the administration of the chloroform; second, as to the best mode of treating dangerous symptoms arising during its administration; and we shall conclude our remarks on Dr. Sansom's work by cordially recommending it as a safe guide on the subject of which it treats:—

"General rules.—I. Examine the condition of the patient's health. I have said how few things preclude the administration; but every case requires care, and every case may require a treatment special and peculiar. A hasty administration, regardless of coincident circumstances, may be safe in the great majority of cases; but, obviously, the more we know about the patient the better. It is better, not only for the purpose of excluding certain cases wherein organic diseases would prove

predisposing causes of danger, but also for the purpose of regulating the steps of the administration. For example, one should, in those of feeble heart, be especially careful to administer dilute atmospheres; in those of impeded respiration, any sign of mal-aëration should be watched. The examination is of especial importance when chloroform is administered for the performance of such slight operations as tooth-extractions, simple incisions, &c.

"II. Be careful that the patient has not had food for about four hours. If in any case it is necessary to relax this rule, take especial care to avoid haste in administration, and if vomiting be induced, encourage it before proceeding further. The influence of the progress of digestion in enhancing the danger of chloroform has been before noticed. I have always thought that a tablespoonful of brandy in water, or a couple of glasses of wine (sherry or champagne) have been of service. It may be that the slight stimulus has dissipated some of the mental apprehension, but I have thought that the value has been something more. In animals I have found, not only that alcohol has diminished the tendency of chloroform to prove fatal, but it has excited the circulation when feeble.

"III. Regulate the position of the patient. The best is the recumbent, but in cases of dentistry and of certain operations about the face there is a necessity for the sitting posture to be observed. The rule should be, let the patient observe the recumbent posture unless the exigencies of the operation point otherwise. The tendency to syncope is greater in the erect and sitting position than it is when the body is horizontal. The pulse in the latter condition is more slow and quiet; thus, though it may be seventy in the former position, it will frequently sink to sixty-five, or even sixty, in the latter. The reason, therefore, that the recumbent posture is to be preferred is that the circulation is more steady and the tendency to faintness is less.

"Whatever be the posture, the rule of Dr. Richardson is most valuable. Let the patient retain the selected position for three or four minutes before inhalation.

"Remove the neck-cloth and any clothing which interferes with the free play of the chest in respiration.

"IV. Reassure the patient. I have sufficiently pointed out the necessity for overcoming fear. 'A few gentle words or a brief conversation, enticing the mind to the consideration of other subjects than danger, or a chance of danger, is invariably a wise and safe policy.'

"V. Commence the inhalation gradually. Use no force if it can possibly be avoided. Persuasion is better than restraint. The value of the gradually progressive administration has been sufficiently shown in former pages. Recollect that any sudden increase in the strength of the vapour can produce sudden syncope.

"The administrator should observe the countenance, the respiration,

and the pulse. There has been too much dispute as to which should receive most attention. Some writers have urged attention to the pulse to the exclusion of the breathing, others *vice versâ*. The truth is, to my mind, that all the signs should receive attention—none more than the others.

"The pulse of itself gives no indication of the stage of the narcosis, but any feebleness or hesitation in it is a sign that the administration should be intermitted.

"The administrator should, it has been advised, keep one hand free for careful observation of the pulse. My own practice is, as I hold the inhaler in one hand, to keep the other upon a branch of the temporal artery. It is easily felt, and thus the pulse is constantly under observation.

"VI. Continue the administration until the necessary stage of narcosis is arrived at. These stages I have divided into three. In the *first stage*—SOPOR (perversion of sensibility) there is no distinct sign of demarcation, but it is measured by the patient's own expression. There is apparently a light sleep, a dreamy half-consciousness, frequently a knowledge of what is being done, and yet an almost complete loss of sensation. For simple incisions, the removal of a single tooth, division of tendons, in cases wherein pain is to be relieved, neuralgia, dysmenorrhœa, and even in natural labour, this stage need not be overpassed.

"In the *second stage*—STUPOR (abolition of sensibility)—there is absence of all consciousness, and a state of quietude. The signs which indicate that this stage has arrived are—(1) The occurrence and subsidence of an involuntary tremor of the muscles in the course of the administration. Usually this is the less violent the more gradually the vapour is administered. (2) The absence of winking when the eyelid is lifted up and the edge of it is touched. (3) The absence of any expression of pain when the skin is pinched or pricked. I believe that the first of these signs is the most reliable, but they should be taken conjointly. This step is the one which it is usually necessary to induce for the performance of a surgical operation.

"In the *third stage*—STERTOR (muscular relaxation)—the signs are snoring respiration and complete flaccidity of muscles. A limb, being lifted, gives no sense of resistence; usually the pupils are dilated. This stage is rendered necessary when dislocations have to be reduced, when relaxation of the muscles is rendered necessary; in operations for hernia, in certain operations in parts which are long in parting with their sensibility (the perinæum, for example), &c.

"Whatever may be the stage rendered necessary, the inhalation should be steadily persisted in till this is arrived at. Only if in the course of the administration the patient makes two or three deep inspirations in succession, the chloroform should be temporarily removed. If stertor

occur, the inhalation should be suspended; and in passing from the second stage to the third an endeavour should be made to intermit it just before the occurrence of stertor, for the symptoms of anæsthesia become more profound for a short time after the inhalation is suspended. The residual amount of chloroform in the lungs, as was shown by Dr. Snow and Prof. Sédillot, becomes absorbed, and thus deepens the existing narcotism.

"VII. When the necessary stage is arrived at allow the patient to breath pure air. If the operation be of short duration no reapplication of the chloroform will be necessary. Usually it has to be repeated at intervals. The proportion last breathed can be then again administered without any more free dilution. Again, there may be a few inspirations of air, and then a reapplication at intervals of thirty or forty seconds. Thus the necessary stage can be kept up for any length of time.

"VIII. Do not be too anxious to reapply the chloroform towards the close of the operation, for frequently there will be a return to partial consciousness, although there remains a complete absence of pain.

"IX. During the time of recovery from the effects of chloroform let the patient be as little disturbed as possible. A natural sleep is the best of sequelæ.

"If vomiting occur it is better at first not to check it. If it become persistent a little brandy and soda-water constitute the best remedy.

"Hysteria, epilepsy, and a sort of catalepsy, have, in a few instances, occurred; these are best treated as ordinary cases; indeed, they may be considered as indirect effects, not due to the chloroform.

"PRACTICAL REMARKS.—If in the course of the inhalation you notice a sign of danger, if sudden pallor occurs, if the pulse fails, if after severe muscular excitement there is sudden collapse, or if there is an evident embarrassment of respiration, at once remove the chloroform, and—

"I. Bring the patient to the recumbent position. The blood regurgitating from the system to the heart may induce in the latter renewed contractions.

"II. With the finger, or with a pair of forceps, draw forwards the tongue.

"III. Make a few alternate pressures by both hands upon the lower part of the sternum.

"IV. *Commence Artificial Respiration.*—Having first brought the patient's arms to the sides, and exerted pressure against the walls of the chest to expel some of the air, lift the arms straight above the head, then bring them again to the sides and compress. Repeat this frequently, but be sure that it is done thoroughly, the arms well extended, and the chest firmly pressed. It may be well to let another press the lower part of the sternum so as to favour expiration.

"Or, if Dr. Marshall Hall's plan of treatment be preferred—

"Turn the body, from the position in which it lies, upon the side, or rather upon the sternum. This compression of the thorax first causes an expiration which forces the residual (chloroformed) air from the lungs.

"[The efforts at resuscitation may be continued by mouth-to-mouth inflation of the lungs; but this, to a certain extent, is subservient to the muscular power of the chest of the inflator. Postural methods can succeed as well.]

"Continue the alternate movement of the body from the posture on the back to that on the sternum twenty times in the minute.

"V. At the same time let *warmth* be applied to the body. Let no cold air circulate near; do not dash cold water upon the chest.

"Let friction be employed, the direction being from the toes upwards.

"If there be a possibility, let a galvanic apparatus be sent for.

"VI. If the apparatus is at hand, place the conductor (covered with wet cloth), which is in contact with the negative pole of the primary wire of the battery, over the phrenic nerve on the right side of the neck, pressing it well in; the other conductor, also wetted, should be pressed into the epigastrium; now set the battery in action for two or three seconds—this will cause instantaneous contraction of the diaphragm—remove either of the conductors for ten or fifteen seconds, and repeat.

"VII. If after five to ten minutes there is no recovery, or if the symptoms indicating danger have been characterized by difficult respiration or coma, perform tracheotomy, but continue your efforts at mechanical resuscitation. Do not relax the efforts, even if no sign of life return, for at the least half an hour.

"VIII. Enemata of brandy and water may be administered during the process; and if the patient recover sufficiently to swallow, a little stimulant may be at once given."

Manual of Materia Medica and Therapeutics. Being an Abridgment of the late Dr. Pereira's Elements of Materia Medica, arranged in conformity with the British Pharmacopœia, and Adapted to the Use of Medical Practitioners, Chemists, and Druggists, Medical and Pharmaceutical Students, &c. By Frederic John Farre, M.D., Cantab. F.L.S.; Fellow of the Royal College of Physicians of London; Honorary Member of the Pharmaceutical Society of Great Britain; Examiner in Materia Medica and Pharmacy to the University of London; London Editor of the *British Pharmacopœia;* Senior Physician to St. Bartholomew's Hospital, and Lecturer on Materia Medica in St. Bartholomew's College. Assisted by Robert Bentley, M.R.C.S., F.L.S.; Honorary

Fellow of King's College, London; Professor of Botany in King's College, and Professor of Materia Medica and Botany to the Pharmaceutical Society of Great Britain: and by ROBERT WARINGTON, F.R.S., F.C.S.; Chemical Operator to the Society of Apothecaries; and Vice-President of the Chemical Society. London: Longmans, Green, and Co., 1865. 8vo, pp. 614, with numerous woodcuts.

THE great work of Pereira must long continue to be a standard authority in materia medica and therapeutics, as it must ever be a memorial, "aere perennius," of the talent, learning, and industry of its lamented author. But its very completeness as a book of reference on the subjects of which it treats renders it both too voluminous and too costly for the majority of readers; and Dr. Farre has conferred on both students and practitioners a real boon in presenting in a comprehensive form, and within the limits of a moderate octavo volume, the more important and more practical portions of his predecessor's great work. That Dr. Farre has spared no endeavour to perform his task in every department in the most perfect manner, may be already inferred from the fact of his having associated with himself in the work, the two distinguished gentlemen whose names appear with his own upon the title page.

To effect this condensation it has been necessary, as Dr. Farre, in his preface, informs us:—1. To omit all remedial agents, except those which the author termed pharmacological—such as mental, physical, but imponderable, and hygienic remedies, or to be more specific, the influence of the mind, of light, heat, electricity, food, exercise, climate, &c. 2. To omit all pharmacological remedies which are not officinal, or contained in the *British Pharmacopœia.* 3. To omit all classifications of medicines except the two which the author himself adopted—one founded on the chemical arrangement of the inorganic bodies, and on the botanical and zoological classifications of the plants and animals which yield the organic bodies; the other founded on their physiological effects. Some other modifications, rendered necessary by the lapse of time and by the progress of science, have, of course, also been made.

Having thus briefly shown, chiefly in the author's words, how the present work differs from its great prototype, we shall now proceed to glance more particularly at its arrangement and contents.

As to the arrangement, the first of the two classifications above

mentioned confer upon the author's table of contents the quality of giving more information than such tables usually convey. Thus, by looking at it, we see at a glance what officinal plants are furnished by each natural family, and what preparations of each plant exist in the Pharmacopœia. We have likewise a list of thirty-one elementary inorganic bodies, and of the preparations derived from each. Products of fermentation, products of destructive distillation, and remedies of animal origin, likewise referred to their respective classes and orders, complete the catalogue.

In speaking of weights and measures, the author suggests that a modified decimal system might have been authorised, and that some such weight as a "dec," or ten grains, and a "cent.," or one hundred grains, might have advantageously replaced the discarded scruple and drachm.

In the body of the work, the author arranges the materia medica in two groups—the *inorganic* and the *organic*—subdividing the former "according to the chemical relations of its members; the latter, according to their external, or, as they are called, natural history characters," The inorganic bodies used in medicine are simple or compound. Of the sixty-six simple or elementary substances at present known, thirty-one enter into the materia medica, &c., of the Pharmacopœia. Of these Dr. Farre commences with oxygen and hydrogen; then treating of their compound, water, in its several forms—as vapour; fluid; or solid, as ice and snow; together with its various uses in drinks, cold injections, baths; in the preparation of infusions, decoctions, solutions, &c.; its employment externally and internally in the solid form; of fomentations, of the inhalation of vapour, &c. A similar plan is followed with the other inorganic bodies.

The portion of the work treating of remedies of organic origin is copiously illustrated with well-executed woodcuts, which greatly facilitate description. The resina podophylli and other new medicines whose merits have secured them a place in the *British Pharmacopœia*, are fully described. The section on the Physiological Classification of Medicines is, like the rest of the work, clear and concise. At the end of the volume is a copious posological table. On the whole, the author has succeeded in condensing within a small compass a large amount of valuable information. The work is neatly brought out in a clear type, and, as we have just remarked, is fully illustrated with excellent woodcuts.

1. *Amputation at the Hip Joint.* By J. SAMPSON GAMGEE. London: J. Churchill & Sons. Pp. 33.
2. *Excision of the Scapula.* By JAMES SYME. Edinburgh: Edmonston and Douglas. Pp. 35.
3. *Resection of the Shoulder Joint.* (Reprinted from the *Army Medical Reports*, Vol. V., 1865.) By Deputy Inspector-General T. LONGMORE, Professor of Military Surgery at the Army Medical School.

WE have grouped together these three interesting contributions to operative surgery. There is much in each of them that will repay perusal; and they have this in common, that they all bear the stamp of genuine work upon them, although each in a special manner of its own.

1. Mr. Gamgee's memoir of his case of amputation at the hip joint is brought out in the highest style of art. Printed on tinted paper, illustrated with numerous photographs, it takes its place at once among the few really handsome books which emanate from the medical printing press. Its appearance alone will secure it attention when many a smaller report is tossed aside into the waste basket unheeded. But it may claim our notice on higher grounds. The case recorded is interesting in itself, and is supplemented by some sound reflections on medical and surgical statistics, and on surgical dietetics. The case was remarkable from the size of the tumour (48 inches in circumference), from its consequent great weight, and the mechanical difficulties thence arising in the performance of the operation, and from the speedy restoration of the patient to good health from the lowest stage of debility.

The size of the tumour when removed and freed from soft tissues may be judged by its length, 29 inches, and its circumference, 46½. It was composed of imperfectly and irregularly ossified cartilage—its weight, 93½ lbs. In these particulars it is exceeded by a case recorded in the fourth volume *Dub. Hos. Rep.*, by Sir Philip Crampton. The enormous size of this may be judged from its measuring six feet six inches in circumference. Mr. Gamgee, however, carries off the palm for his success in removing the tumour by amputation at the hip joint under circumstances which might have deterred most men without imputation of professional cowardice. We must refer our readers to the work for the details of the case and the operation.

From the remarks on Medical Statistics we cull the following as a specimen of Mr. Gamgee's style:—

"In medicine, as in other departments of knowledge, the greatest errors have been promulgated on the alleged foundation of facts. To test the value of such teaching, two inquiries have to be instituted—1stly, into the reliability of the individual facts; 2ndly, into the soundness of the method of arranging them, and reasoning upon them. The mere enunciation of a proposition, on the basis of a number of facts, is repeatedly taken as evidence of proof, but on the slightest reflection it must be evident that it is not so. Great as was the value of the aphorism in Morgagni's day—*observationes perpendendæ non numerandæ*—it is not applicable to our time; the copulative must now be substituted for the disjunctive, and the sentence then accurately expresses the order to be followed in the process of inquiry—observations of fact have to be well weighed and then counted."

2. Mr. Syme's little book gives a graphic account of two cases in which he removed the scapula, and a third in which he removed it along with the upper extremity. The latter case succeeded, as did one of the former, and the other patient died after two months, as much, probably, of old age (70 years) as of the operation. The cases are detailed in Mr. Syme's terse, vigorous style, and the conclusions he draws from them are as follows:—

"1st. That the entire scapula, either alone or together with the arm, may be removed without much difficulty or loss of blood.

"2nd. That the wound thus inflicted may heal quickly and soundly.

"3rdly. That the arm, if preserved, may be strong and useful."

To which we may add his conclusion of the whole matter:—"I trust that excision of the scapula will be recognized as a legitimate and established procedure of surgery."

3. Mr. Longmore's report is well written; the subject has been previously well studied by him. It is entitled, *Remarks upon the Cases of Six Invalids Admitted during the Year* 1864, *at the Royal Victoria Hospital, upon whom Resection of the Shoulder Joint had been Performed, in New Zealand, for Gunshot Injuries.* The statistics which he has got upon the subject of resection of the head of the humerus are favourable to the operation so far as they go, but further observations are needed. The results of the operation are, on the whole, as favourable as those of amputation at the joint, with the further advantage of preserving an arm of more or less usefulness.

The Principles and Practice of Medicine. By JOHN PEET, M.D. 8vo, pp. 590. London: Churchill. Bombay: Thacker, Vining, and Co. 1864.

A MEDICAL work from our Indian brethren we shall always be disposed to hail with welcome. What a field is not there presented for original observation and inquiry amongst the vast, varied, and exceptionally circumstanced population of the great Indian peninsula. Again, how much have we not yet to learn of the comportment of the European constitution under the conditions which the soldier's or the civilian's life presents in the far East.

Dr. Peet's work is chiefly for students, but "may perhaps be found in some degree useful by those medical officers who are for the first time entering upon medical practice in India." It is to be regretted that writers of practical works so often think it necessary to prefix an "introduction," or a series of chapters on "diseased states of the blood," "derangements of intestines," &c., in which it is attempted to give concise views of the present condition of various branches of general pathology. The all but invariable result is a superficial enumeration of the heads of subjects embracing a very wide range of physiological, pathological, chemical, and general physical inquiry. No exact or sufficient information is conveyed to the student who approaches any of these topics for the first time; and to the educated practitioner this part of such a work is perfectly useless. Dr. Peet has sinned in this regard, but not perhaps more grievously than many of his compeers and predecessors, in the field of practical medical literature. We regret that the time thus occupied is not more profitably turned to account with the detail of the author's own observations on disease as he has seen it in India. Indeed, in the section devoted to the description of individual maladies we miss the clinical record of disease, and fail to find in Dr. Peet's rapid summary of symptoms that power of delineation which brings disease to the mind's eye of the reader in all its vitality.

We regret not to be in a position to speak more favourably of a work from the pen of an Indian medical officer, and one who, in the great Jejeebhoy Hospital, of which he is surgeon, must enjoy such ample opportunities for the study of disease.

Register of the King and Queen's College of Physicians in Ireland; with Historical Introduction, and an Appendix containing a Roll of the Presidents and Fellows from 1654 *to* 1866, *an Obituary of Honorary Fellows, Candidates, Licentiates, and Licentiates in Midwifery, from* 1692 *to* 1866, *and a Catalogue of Pictures, Busts, etc.* Corrected to January 1st, 1866. Pp. 128. Hodges and Smith, Grafton-street.

In the number of this Journal for August, 1865, we noticed in favourable terms the edition, for that year, of this work; and while renewing the commendation, we on that occasion were in justice bound to accord, we have at present to notice many important and interesting additions, which, in the "Historical Introduction" are most numerous. We are glad to notice (at page 16) that the number of licentiates has increased from 597 to 862, and we feel assured the next edition will have a further progress to record in this respect. We find ten pages devoted to information concerning the present working of the various departments of the College; the enactments touching "The Library" are well stated, and since the removal of the books, &c., to the present building, and the appointment of a librarian, nothing further is to be desired; a full account is given of "The Reading Room," established in 1864 for the use of the Fellows and Licentiates; and of "The Museum," which was founded in 1861, and consists of specimens of the Materia Medica, presented to, or purchased by the College, with a small collection of medals. "The Medical Society" instituted in 1864, a revival of the Association of the College of Physicians, founded in 1816, "for the purpose of more intimately uniting the Fellows, Honorary Fellows, and Licentiates," is also noticed; and the articles on the "School of Physic" and "Sir Patrick Dun's Hospital," contain a short but valuable account of those institutions, in the past and present time; whilst under the head of "Examinations," we find the rules and regulations in force at present; but we believe the examination in the modern languages has been abolished since the publication of this edition. As the chief additions are to be found in this portion of the work, under the above heads, we have only to add that the remaining portions of the book have undergone careful revision, and the entire work is a faithful and correct index of the King and Queen's College of Physicians. To mention each of the thirty-five articles, so well described in the "Catalogue

of Pictures, Busts, &c.," contained at the end, would far exceed our limits, we shall, therefore, in conclusion, merely recommend this edition to the profession as worthy of that consideration and support it is sure to get, and so well deserves.

The Aural Surgery of the Present Day. By DR. W. KRAMER, Berlin. With Two Tables and Nine Woodcuts. Translated by H. POWER, M.B., London. With corrections and numerous additions by the author. New Sydenham Society. 1863. Pp. 154.

Ohrenkrankheiten und Ohrenärzte in England und Deutschland. Ein Nachtrag zur Ohrenheilkunde der Gegenwart, von DR. W. KRAMER, Geheimer Sanitätsrath. Berlin: 1865. Pp. 96.

Aural Diseases and Aurists in England and Germany; being a Supplement to Aural Surgery of the Present Day. By DR. W. KRAMER.

WE confess to feeling somewhat uncertain as to how we should notice these volumes. As they have been sent to us with the special compliments of their author, we feel bound to record our opinion of them, and yet we know not from what stand-point to view them. Are they meant as genuine contributions to aural surgery, or are they meant as a strange, and in this country, a novel experiment to demonstrate to what an extent and under how distinguished a patronage the professional peculiarities of an individual can be advertised?

We have read the *Ohrenheilkunde der Gegenwart* with feelings of amazement; and we felt surprised that any respectable English society would have mispent its money, and misapplied its patronage by translating so feeble a work; but we hardly know how to characterize our feelings, on perusing the supplement to this work, in which members of our profession, of undoubted standing and character, are alluded to and spoken of as ignorant quacks and charlatans.

The first of these works, that translated under the auspices of the New Sydenham Society, has been so fully criticised by the author of *A Vindication of the Present State of Aural Surgery*, that we shall content ourselves with adopting his opinion of it, and

asserting that "instead of being, as its title would indicate, a comprehensive and intelligent survey of the present state of aural pathology, it is in reality little more than an advertisement of the author's peculiar system of treating diseases of the ear by means of his four catheters and catgut bougies. Anatomy, pathology, and scientific surgery are sneered at; every modern plan for simplifying the study of ear diseases and for enabling them to be diagnosed and treated by the members of the profession are wholly ignored, and the author's system of using four catheters and catgut bougies is made to be the *all-in-all* of aural diagnostics and therapeutics."

With reference to the supplement, we shall examine it somewhat more in detail. Through peculiar circumstances, Dr. Kramer tells us, it so happened that during 1861–64 he spent some three or four months of each year in London, following his profession as an aurist. We may tell our readers, what Dr. Kramer does not tell them, that the period of his arrival, the place of his residence, and other details were duly advertised in the daily press; and while we would be slow to measure the conduct of a Prussian surgeon by what may be considered our own refined standard of etiquette, yet we think Dr. Kramer must have been fully aware of how unprofessional such a proceeding would appear in medical eyes.

Its results, however, appear to have been favourable, and patients resorted to the Berlin aurist in numbers. It is tempting to dwell on this subject; who does not know the mighty advantage that accrues to the medical man by his having been in foreign lands, and how intensified this advantage becomes if, by some lucky chance, he either is, or becomes, altogether metamorphosed into a foreigner. One would have thought that a great and enlightened public would by this time have found out that almost every medical man, now-a-days, has had the privilege of studying in some foreign school, and that the fact has become so common that it need not excite either their admiration or surprise. Perhaps it is coming to this; but the belief in a foreign education is still strongly held, and we must be still content in this respect to class ourselves with the *artistes* of at least an equally harmonious profession, thinking ourselves fortunate that, unlike them, we are not as yet obliged to affix an Italian termination to our names.

To lead the reader, however, to imagine that Dr. Kramer had any such faith in foreign doctors would be to seriously misrepresent him. Utterly despising all the continental aurists, he has by his

visits to London been brought into such close contact with the literary and practical activity of the English, and allow us to add of the Irish aurists, that he very soon learnt to think very little of them indeed; so little that it was not enough that he should have indirectly alluded to their errors in his *Aural Surgery of the Present Day*, but that he felt compelled to publish this supplement, in which he tells us of some 300 cases of ear diseases which he had seen in England; and then proceeds to show how little every living aurist but himself knows of ear diseases, especially those of the middle ear, and how grossly ignorant they are of the use of the Eustachian catheter; not only is Toynbee in this respect a "wretched aurist," but as to Wilde, his very assertion that he uses such an instrument deserves no credence whatever (Seine Versicherung den Katheter "in vielen Fällen anzuwenden" verdient desshalb auch gar keinen Glauben); and so with Von Troeltsch, Erhard, Voltolini, Schwartze Weber, Politzer, and others. It is so easy to verify these statements that even at the risk of being a little tedious we will venture to give a few select extracts from the criticisms on some English aurists, whose works will be easily accessible to the reader, and so the justice of the criticisms be the more easily comprehended. Nobody but Dr. Kramer would appear to know much, if anything, about diseases of the middle ear, and most elaborate and often times very unfair attempts to prove this are made in his supplement. According to Dr. Kramer, "Toynbee treats very superficially of chronic inflammations of the tympanic cavity; indeed so much so that his views need not be especially criticised; but while he talks of 'rigidity' of the mucous membrane of the tympanic cavity, he in his 'Descriptive Catalogue' details as the result of *post mortem* examinations only such appearances as 'swollen,' 'reddened,' and such like, but never 'rigidity.' How comes it then that Toynbee, who is so proud of the pathological-anatomical ground-work of his system of aural diseases, can treat of such a form of disease as 'rigidity of the mucous membrane of the tympanic cavity,' when he never yet proved, by his anatomical researches, that such a state of things existed; this does not, however, prevent him from stating that the diagnosis of this form of disease is by no means difficult. 'One has but to inspect the tympanum, which is often but little, if at all, transparent! sometimes even swollen!! The Eustachian tube admits the air either with its usual or very slightly altered sound.' Equally trifling are the subjective symptoms—'inability of understanding the conversation when several

people speak at the same time, or when one speaks too rapidly—or inability of appreciating the sounds, though the voice is heard; such symptoms, and the history of the disease will show that the case is one of hardening of the mucous membrane of the tympanic cavity! and when one takes into account the absence of all the symptoms indicating nervous deafness, will enable the attentive observer to form an accurate diagnosis.' Such a conclusion makes Toynbee's *naivète* (not to use a stronger expression) really perfect. Of course the curing of such a disease thus so imperfectly diagnosed offers no difficulty whatever to Toynbee." "One must be of a very easy and complacent turn of mind indeed, to indulge, like Toynbee, in such phantastical views of such hypothetical ear diseases, and to treat thus of their nature, diagnosis, and curability." "Again, Toynbee writes of other pathological changes met with in the mucous membrane of the tympanic cavity, such as 'membranous bands in the cavity;' but then without treating of any attempts at diagnosis, he gives the practical surgeon the very discouraging information that 'during the lifetime of the patient this pathological condition cannot be diagnosed from that of rigidity of the mucous membrane;' so that when we remember that pathological anatomy has thrown no light on this latter disease as met with in the living subject, and that nothing can be done for its permanent cure, it is very obvious that quite the same must be said about this latter affection. It is therefore somewhat satisfactory that Toynbee, with his accustomed confidence, declares that the treatment of both diseases being the same, it is not of much consequence to distinguish the one from the other during life! This gentleman possesses indeed a most enviable amount of self-confidence!! For the grossest inconsistencies he does not care in the slightest!! For example, with, and in spite of the declaration that bands of membrane cannot be distinguished during life from rigidity of the mucous membrane, and thereupon cannot at all be diagnosed, he says:—'We should take care to distinguish between bands formed by organized membrane from those consisting of dried up mucus; of course he never stops to indicate the diagnostic mark between them, but then the treatment is the same. Spanish fly-blisters, stimulating blisters, and such like, will in from four to six weeks cure everything—be it rigidity of the membrane or membranous bands, organized or consisting of but hardened mucus."

Anchylosis of the stapes to the fenestra ovalis is also alluded to in detail, and an abstract of Mr. Toynbee's statistics on this subject are

given, and criticised in the usual strain:—"While stating that the diagnosis of this form of disease causes little difficulty in the majority of cases, if one only carefully examines into its origin and progress, and properly inspects the ear with the necessary accuracy, he shows by the analysis of his five selected cases how little he acts up to his own advice; the details of these cases showing little but the 'slow development of deafness,' not an uncommon feature in many different diseases of the ear; and so far as the accurate examination of the ear itself is concerned, Toynbee has, in four cases out of the five, altogether neglected it, and in the fifth simply inspected the membrana tympani, and tried the method of Valsalva. Such trifling inconsistencies do not, of course, flutter a man like Toynbee, who goes even still further, and points, in his diagnosis, to the striking similarity which exists between the symptoms of this kind of disease and those of rigidity of the mucous membrane of the tympanum; but we saw that this purely hypothetical disease was not itself to be distinguished from membranous bands in the tympanum, so that we can take our choice as to which of the three diseases our patient labours under; not that to the practical surgeon it need make much matter, as Toynbee makes the consoling declaration that the treatment is the same in all these cases. Under such peculiarly convenient circumstances it is simply as a luxury that I enumerate here—just for curiosity sake—the special symptoms by which Toynbee, after all, teaches us to recognize this anchylosis. They are 'slow development of deafness, dryness of the auditory passages, singing in the ear, hearing better while driving, reddish swelling of the membranous auditory passages, enlarged tonsils, membrana tympani thickened or often not! etc., etc., all symptoms, that from their vagueness, and after what I have written about Toynbee's diagnosis of diseases of the middle ear, cannot be considered by any aurist who has common sense left, as characteristic of anchylosis of the stapes to the fenestra ovalis; and when, in conclusion, Toynbee points out that rigid attachment of the basis of the stapes is really capable of being much ameliorated; when he even speaks of there being an advantage in the stapes having an expanded basis, obtained through the resorption of the thrown out bony material, so that only such cases, where there is a bony coalescence between the articulation of the stapes and the vestibule, appear to him really hopeless; then, indeed, it is nothing but ridiculous charlatanery (ganz lächerliche Charlatanerie!). The gentleman has really no knowledge whatever of the existence of

all these conditions in the living subject, how then can he know whether they have ever been cured, or even improved; and, at all events, the preceding observations justify me in abstaining from all further examination of Toynbee's writings, and justify me in stating that while he has dissected very many ears, and made himself acquainted with their pathological conditions, yet as he has not seen these ears during life, the result of these dissections are lacking of any real connexion with the pathogeny of deafness. They are nothing but an accumulation of bare unconnected facts, and Toynbee's merit for the mere multiplication of such is very light; and hence not only does he himself remain a bad aurist (ein schlechter Ohrenarzt), especially with regard to diseases of the middle ear, but he has also been the means of misleading the younger German aurists into a similar superficiality in their description of diseases of this part. None, however, of the living German aurists have sunk so low as Toynbee, because none of them, except Schmalz, have neglected, as much as Toynbee has, the study of ear catheters."

"I know quite well that my opinion of this gentleman will call forth a cry of indignation from his many followers, as Erhard, V. Troeltsch, Voltolini, Politzer, and others. I, however, entreat these gentlemen, beforehand, to abstain from mere empty declamation on this subject. My opinions are based on numerous grounds, and whoever cannot confute them must take the consequences."

Perhaps the reader may by this be amply able to judge of Dr. Kramer's style; but as the next name called up for judgment is one very well known to them, we, even at the risk of taking up a little more space, venture to give a few more extracts. A good story is told of an old rector who, when called upon to preach before the members of his university, unfortunately selected a sermon long before preached in a remote country parish. The text was good, and the introductory matter not bad, and then the discourse sprouted, as such discourses generally do, into two heads; the firstly was to the young, and the college lads sat trembling; their seniors complacent awhile, while warnings, and threatenings, and denunciations were launched against them—poor unoffending souls; but the secondly came, and the wonder of all was great and the surprise terrible when the preacher read—'but as for you, you hoary-headed sinners, how shall I warn you from the wrath to come.' So Toynbee may be an ignorant boy, but Sir W. Wilde is an old offender, and it is not by accident that Dr. Kramer takes care to tell him so:

"He is the author of a work on aural surgery, but he never himself dissected a single diseased ear. In his lamentable neglect of the ear catheter and in his groundless confidence in the diagnostic value of Valsalva's method, he, in point of time, precedes Toynbee. He reduces the examination of the symptoms of the diseases of the middle ear to recording whether the tympanum can be inflated or not, as observed by Valsalva's method, and to the observation of the audible or *inaudible* sounds, although the latter are designated by Wilde as 'unimportant.'" This is really good; but, however laughable, it simply shows that Dr. Kramer has not only not understood this portion of *The Aural Surgery*, but has most positively misquoted it. In justice, we feel bound to give the sentence entire:—"If the patient be able to inflate the tympanum we may apply our own ear, either directly or through the intervention of a stethoscope, and the peculiarity of sound which is thus produced, *i.e.*, by the inflation in the middle ear, either the ordinary normal rush of air or a prolonged squeeling or gurgling sound, such as might be caused by any contraction in or thickening of the walls of the Eustachian tube, or by dryness, or by the accumulation of mucus, either in it or in the cavity of the tympanum, is heard; the stethoscope should also be applied over the mastoid process, and the same series of observations made upon *the sounds, if any, produced there;* but these latter (evidently referring to those heard near the mastoid process) can seldom be heard distinctly."

"Wilde, for courtesy sake, it is true, calls the ear catheter a valuable means of diagnosis; but he soon afterwards corrects himself, and says:—I have latterly found it very seldom necessary to resort to its use. It is therefore more than probable that Wilde never uses it now-a days, and this is rendered the more probable because in the detailed examination of some two hundred patients ear catheterism is not once mentioned; but even in these seventy-five cases it is by Valsalva's method that the middle ear and Eustachian tube were found uninflatable; his assertion therefore that he uses the catheter in many cases deserves no credence whatever. What may be his reason for thus neglecting this 'valuable means of diagnosis,' can scarcely be stated, if it be not entire want of skill on his part to use it; indeed Wilde's thinking it quite sufficient to be provided with a single silver catheter, his talking of its being easily shifted by speaking or swallowing, and his stating that the passing of a catgut bougie through the Eustachian tube is impossible, even in a dead subject, are clear proofs that he does

not know how to handle the instrument, and is but hunting for reasons why not to use it." Many such assertions are met with. At one time we read that "Wilde's statements savour more of uncertain surmises than of full convictions;" again, "Wilde records cases which are most unsufficiently diagnosed, indeed in some instances he gives cases having no results, and altogether unworthy of mention, and this though he calls them himself 'excellent cases,' and though he be, like Toynbee, a professor and teacher of aural surgery."

"As Wilde himself tells us that it is impossible to diagnose accurately the pathological conditions of the tympanum, it would be useless to criticise his negative diagnosis of nervous deafness; but he seems to admit the insufficiency of his method of examining such cases, and in this we agree with him, believing that to him all diseases existing on the other side of the uninjured membrana tympani are but as a sealed book or as an enigma about which he knows nothing, and for which he yet applies vesications, corrosive sublimate, &c."

We have thought it right to justify the opinion we have expressed of this book by thus giving very briefly an account of how two very able and eminent surgeons, both of them well known to our readers, have been criticised and condemned by Dr. Kramer. All the other names we have above referred to are equally severely and unjustly condemned; but the instances already given will be amply sufficient to give an idea of the general tone of this supplemental volume; and in taking leave of its author, we wish to tell him that while we would be the very last to put an end to free discussion, yet that we draw a well marked distinction between criticising an author's views, as they are to be met with in his published writings, and publishing a work which hardly contains a word of true criticism from its beginning to its end; but consists in an attempt to prove that no one is capable of performing a not very difficult surgical operation but himself, and that of all aurists he is the only one that knows his business.

This supplemental volume will probably not circulate very much, at least in its present form, in this country. The *Aural Surgery of the Present Day*, on the contrary, had a very extensive circulation, having been introduced to the notice of British medical men under the auspices of the New Sydenham Society. There can be no doubt now that in thus doing the New Sydenham Society committed a very great mistake, and that it to a certain extent lowered

its standard of merit by undertaking the sponsorship for such a volume.

In catering effectively for a large number of subscribers it must always be a most difficult task to please even a large majority, it must be a hopeless one to please all, and it is far from us to say one single harsh word against a Society which already has published so much that is excellent and good; and yet we think there are very manifest signs of indisposition to be found in this Society, and that unless its energetic secretary—who is at the same time its most active and worst paid officer—does not do something to arouse it from its present unsatisfactory condition it will be very likely to soon go the way of its progenitor, the Sydenham Society of 1843–'56. In our opinion, the peculiar functions of such a medical and surgical society ought to be to keep us acquainted with all the best modern literature of the day; now it is notorious that to the greater number of our medical brethren, German medical literature is unknown, and to a less extent French literature also, and it is equally a fact that many medical memoirs published in Austria, Prussia, Bavaria, and Saxony would, if properly translated into good English, be most acceptable to our medical men. For every plate representing some skin disease we might have some recent monograph on some important subject by Hebra, or Virchow, or Langenbeck, or many such, translated and published; and there is this to be borne in mind, that such publications are not accessible at present, whereas plates of skin diseases are; those by the late Dr. Elfinger, still publishing in Vienna, can be easily obtained by those who want them; and while they may not surpass, they certainly equal, anything of the kind published by the New Sydenham. Suppose we put the case this way:—A fasiculus of plates of skin diseases gratifies one hundred subscribers, will there not be at least three hundred gratified if monographs on different subjects, of course varying in size, were published instead. As to the *Year Book*, we have always regarded it as an important publication. But the compilation of a year book must in this country always be an expensive task; so, as a matter of expediency, we are glad to see its publication is to be but biennial; and unless when the matter recorded is very remarkable or novel we trust there will be no extracts.

We wish the society every success; that it should make mistakes now and then is what, of course, we must expect; that it made one in publishing Dr. Kramer's *Year Book*, we fully believe; but we would suggest that it is another mistake not to cease the publication

of its *Atlas*, which, after all, benefits but a small section of its subscribers and a London artist or two; and we would recommend it to take upon itself more the functions of a translating society—translating, however, nothing but good works, and leave the publication of original matter or plates to other and somewhat more legitimate channels.

A Description of the Diseased Conditions of the Knee-joint which Require Amputation of the Limb, and those Conditions which are Favourable to Excision of the Joint, &c. By PETER CHARLES PRICE, late Surgeon to the Great Northern Hospital. Edited, with a Preface and Memoir of the Author, by HENRY SMITH, F.R.C.S., Assistant Surgeon to King's College Hospital. London: John Churchill and Sons, New Burlington-street. 1865.

THE posthumous works of an able man must always claim a peculiar attention. Speaking to us as it were from the grave much of the ordinary feeling with which they would otherwise be perused will of necessity disappear, and will be replaced by a special reverence. Criticism of style will not be thought of; imperfections and deficiencies will attract less remark; and the dicta, which under other circumstances would not have weighed with us, obtain an almost Delphic value. When a surgical work so circumstanced is still further commended to our notice by the exertions and affectionate sympathy of one who was not only a personal friend of the deceased author, but who has also made for himself a reputation in surgery, we feel that we are bound to give its contents more than ordinary attention. For this purpose, finding ourselves debarred from ordinary criticism, we shall endeavour to give a short analysis of the work before us, in the hope that our readers, who are interested in the important branch of conservative surgery, which forms its subject, will be induced to seek a closer aquaintance with it.

The circumstances which gave rise to this work are related in the short biography, which Mr. Henry Smith has written of its author:—

"The Royal College of Surgeons had announced for 1860-61, the following title as the subject for the Jacksonian Prize Essay:—'A description of the diseased conditions of the Knee which require

amputation of the limb, and those conditions which are favourable to excision of the joint; with an explanation of the relative advantages of both operations, as far as can be ascertained by cases properly authenticated.' And to use the eloquent words of his friend and biographer, Dr. Webb, 'With his wonted energy Price threw himself into the work. He sacrificed his leisure, and even his rest, to it; and he produced an essay on excision of the knee-joint, which for the fulness with which the subject is treated, the vast number of facts brought together, the beautiful preparations, drawings, and photographs which accompanied it, is unequalled in the records of conservative Surgery.'

"An immense amount of labour was bestowed upon the compilation of this Essay; it entailed not only a large extent of personal research and observation, but a large correspondence was necessary in collecting data connected with the operation, from all parts of the United Kingdom, from the Continent, and from America. Knowing, as he did, that few besides himself could have had the same opportunities of observing facts in reference to this proceeding, he looked forward with the utmost confidence to the result of the award. Those few of his friends who knew what he was about, were equally sanguine with himself. And I can truly state that it was from the knowledge that Price was writing for the prize, that there was no other competitor for the same. A very distinguished and well-known fellow pupil had already collected material for the Essay; but on its being represented to him that such a formidable adversary as Price was working for the prize, he gave up the task.

"After months of labour, both night and day, the Essay was completed, and sent into the College, accompanied by numerous illustrations and preparations. At this period, viz. 1860, the time-honoured custom of re-election to the Council—the '*imperium in imperio*'—had not been broken through, and there were but few members of this august body who practically knew much about conservative Surgery. Such men as James Paget, Henry Hancock, and William Fergusson, had not a seat there then, unfortunately for poor Price. Will it be believed that, of the three gentlemen who were appointed to adjudicate for the Prize, only one of them had ever performed the operation of excision of the knee-joint! Most of my readers will agree with me, I think, in stating that the Council of the College of Surgeons had no right to propose such a subject for the Jacksonian Essay, if they could not find amongst their body three surgeons who were capable, from practical experience, of adjudicating upon such a much-vexed question as the relative merits of curing disease of the knee-joint [*sic.*] by amputation or excision.

"The result of the deliberation of the three gentlemen appointed by the Council was, as might be pretty well anticipated under such peculiar circumstances, that the Essay was returned to its author as not worthy of their acceptance; and this, *notwithstanding that no other essay was sent in.*"

The rejection of his essay, under such circumstances, bore heavily on poor Price's health and spirits; and although his well-known industry and ability were rewarded, not long after, by the appointment of assistant surgeon to King's College Hospital, he never regained his physical strength, and in 1864 he died of pulmonary consumption. There is no doubt that by his death the surgical profession sustained a great loss in the person of one of its most gifted and promising members.

The want of a table of contents and index is very insufficiently supplied by the following analytical sketch of its subject, furnished by Mr H. Smith in the preface:—

"In the first part the anatomy of the knee-joint is described; then is given at length a sketch of the various diseased conditions which demand the operation of amputation or excision. The history of the latter proceeding, both in this country and abroad, is next furnished. Then is given the analysis, with details of all the cases which have been performed in England, as well as of those which were operated upon in Germany, France, and America. Next follows a record of the cases which have occurred in Military Surgery. We are also furnished with an analysis of the fatal cases occurring after this operation. He makes an elaborate inquiry as to the cases to which the operation is applicable, and the circumstances under which it should be performed, and enters minutely into the objections advanced against the proceeding. In the latter part of the work will be found a consideration of these cases of disease of the knee-joint, which are to be treated by amputation, with the respective mortality after each operation; and lastly, the author sums up his own deductions, and gives us the result of his own experience in detail."

Over part I., which contains a general description of the anatomy of the knee-joint, we need not linger.

Part II. gives a short, and on the whole, satisfactory sketch of the various diseased conditions of the structures comprising the knee-joint, which, under certain phases, demand a recourse to amputation of the thigh, or excision of the knee-joint. This part, in fact, constitutes the bulk of the volume, and will be perused with much interest.

The subdivision of this chapter are as follows:—I. Diseased conditions of the synovial membrane. In this paragraph, which deals of acute synovitis, there is a good description of the affection, but nothing specially requiring notice. The same remark applies to II. On sub-acute inflammation of the synovial membrane. III. On Chronic synovitis contains the following:—

"Swelling of a definite form is one of the earliest and most clearly-defined symptoms; while the absence of pain, and even tenderness, is often so remarkable, considering the amount of apparent mischief, that their non-existence is sometimes sufficient to deceive the surgeon and patient as to the exact state and progress of the disease. I believe that there are few surgeons who will not readily admit that pain is a most equivocal and deceptive accompaniment of joint diseases. I have over and over again been deceived as to the nature and extent of diseased action invading the knee-articulation, because I have looked upon the presence or absence of acute pain as a chief diagnostic feature; and in recording instances in which excision has been performed, it will be shown that extensive destruction has often taken place in the joint without the occurrrence of any very considerable pain or annoyance."

We believe, with Mr. Price, that the presence or absence of pain is no criterion of the actual condition of a joint which has been for some time the seat of chronic synovitis; but we must add, in explanation of or addition to the above, that the amount of mischief is generally proportioned to the aggregate of pain which has been suffered from time to time, and that fresh mischief is always accompanied by fresh attacks of pain and heat in the joint: the pain may be dull and intermittent, and the heat may not be very great, but both can be detected; and if detected, generally subdued by cold or tepid bandaging, using opiate solutions to moisten the cloths, by cautious leeching, and by avoiding irritants and counter-irritants. At the same time there is no denying that repeated though trifling accesses of inflammation in these chronic cases are the rule, and tend, unless very sedulously observed and combated, to complete disorganization of the joint.

IV. On pulpy and gelatiniform degenerations of the synovial membrane recognizes the position of this particular form of disease as a fibro-gelatiniform destruction or degeneration which quickly implicates the cartilages and other component tissues of the joint. This affection is, in fact, the meeting point of destructive inflammation of a chronic character, implicating at once the synovial membrane and the cartilages; as such it is fitly placed before V. On diseased cartilages. Here we have a due acknowledgment of the researches of Toynbee, Goodsir, and Redfern into the pathological alterations of cartilage, as correcting previous errors on the subject. The following extract gives a good idea of our author's facility of condensation:—

"With regard to this so-called ulceration, Dr. Redfern clearly observes that the process in cartilage and other tissues is exactly the same, if we regard merely the actions of the essential elements of the textures, although the points of dissimilarity consist in the non-production of inflammatory products, as exudation and purulent secretions, and the absence of all painful sensations. But the process of destruction, pathologically considered, consists in a special granular degeneration of the component cartilage cells; and the shredding and loss of tissue which obtains, is due to apparent absorption and destruction of the hyaline substance, which splits up and softens into a gelatinous and finely molecular material. These changes take place in the majority of all serious diseases of the knee-joint, although the exact nature of the cell transformation varies, and instead of presenting a granular appearance exhibits a fatty or fibrous consistence. Such variations are, however, dependent on the character of the morbid actions; for while in acute and chronic inflammatory destruction of the joint, the granular change predominates, that of a fatty nature results in such instances in which the articulation, although not positively or seriously diseased, has remained unused for a considerable period. When a gouty or rheumatic taint is plainly visible in influencing joint mischief, the articular cartilages undergo a decided fibrous degeneration. The same transformation of the cartilage-cell is apparent in the cartilages of elderly people, and in such conditions in which the synovial apparatus has been more or less chronically destroyed or removed, and the articulating ends of the bones have become susceptible of pressure. To see this fibrous degeneration it is only needful to examine a portion of cartilage on which mechanical influences have acted."

Mr. Price has also recognized the fact, that the so-called ulceration of cartilage is often the result of serious inflammation of the sub-cartilaginous lamella of bone, and of the adjacent cancellous structure; he also has been able to dissociate pain and starting of the limb from ulceration of the cartilages, and to assign to it, as its proper cause, the above-named implication of the bone. The section on diseased conditions of the articular extremities of the bones composing the knee-joint is too brief—in fact all through these sections there is a constantly recurring *desiderium*, that the author had lived to convert his intended prize essay into the mature and perfect monograph. There is material enough here on which to form the opinion that had he lived to do so the surgery of the knee-joint would have been written in better style than has yet been attempted. Favourable to resection his verdict would, probably, have always been, but we have no doubt that with more extensive experience would have come a clear and just appre-

ciation of its position as an operative proceeding, and a scientific discrimination of the cases to which it is suited. To our mind, a perfect work on the subject of resection has yet to be written.

Want of space compels us to pass over without notice many minor sections on tuberculous disease of the articulating ends of the femur and tibia, on tumours, wounds, and deformities demanding surgical interference, &c. The history of the operation is now familiar to our readers. Mr. Butcher, whose name is honourably mentioned by Mr. Smith, in common with those of Sir Wm. Fergusson, Mr. Jones of Jersey, and Mr. Price, as having made excision of the knee a legitimate proceeding in surgery, has by his repeated essays on this subject left nothing for us to extract from the book before us on this point. The statistics of the operation are, of course, more fully given by Mr. Butcher, although not in a form handy for reference. There is a slight difference in Mr. Price's tables and Mr. Butcher's as to arrangement: the latter proceeds chronologically, while the former groups all the cases of each operation together. Each method has its advantages, but neither in their present form is perfect In practical deductions from his statistics Mr. Price shows forth as the truly philosophical surgeon. For many of these we must refer the reader to the work itself; some extracts we cannot forbear to make:—

"In the foregoing synopsis and *résumé* of 291 cases, in which the knee articulation has been excised for disease, deformity, and accident from the year 1762 to the end of (middle of December) 1860, not only in this country, but in America and on the Continent, I have endeavoured to state,—*Firstly*, the exact number of cases with which I have become acquainted; *Secondly*, to give the name of each operator, the number of times he has performed the operation, and the result of his cases, also the nature of the mischief which necessitated the interference. In this way, although not without considerable difficulty, I have contrived to deduce the various statistics which, I have every reason to believe, are as correct as can be obtained with any degree of certainty.

"Analysing still more minutely these 291 cases, we obtain some interesting information. These 291 cases have been divided among 106 different surgeons,—*i.e.*, that only 106 surgeons, during a period of 102 years, have resorted to the operation.

"But to go further. It will be found, as already stated, that these 291 cases may be divided into sections, in accordance with the time of their performance and the residence of the operators, thus:—From 1762 to 1830, seventeen cases were operated on, with ten deaths.

"Of the nine cases operated upon by Germans from 1830 to 1849, four died.

"Of the twenty-one cases operated upon by continental practitioners from 1850 to 1860, nine died; and of 238 cases operated upon by English, Irish, Scotch, and Jersey surgeons, fifty-five died; while in six cases operated upon in America none died, so far as I am aware.

"To reduce the observations to a tabular form, it will be found that—

	Number operated on	Cured	Died
From 1762 to 1830 (mixed) . .	17	7	10
From 1830 to 1849 (German) . .	9	5	4
From 1850 to 1860 (Continental) .	21	12	9
From 1850 to 1860 (British) . .	238	183	55
From 1852 to 1860 (American, . .	6	6	0
Totals . .	291	213	78

"It has already been stated, somewhat in detail, from what causes all the fatal cases sank; but, as I have not found it expedient to give in detail every case operated upon in the table of 238 cases, embodying all the instances of the operation which have come under my notice, as performed by British and Jersey surgeons, I shall now offer a tabular form of the causes of death in fifty-five unsuccessful cases, giving these causes in a numerical order.

Causes of Death.	No. of Instances.
Exhaustion, &c.	14 Cases.
Pyæmia	10 ,,
Phthisis	5 ,,
Irritation, hectic, and secondary hæmorrhage .	5 ,,
Shock	5 ,,
Causes uncertain	4 ,,
Chloroform and amylene combined with shock .	3 ,,
Pleurisy	1 Case.
Pleuro-pneumonia	1 ,,
Suppression of urine	1 ,,
Dysentery	1 ,,
Diarrhœa	1 ,,
Anæmia	1 ,,
Erysipelas	1 ,,
Peritonitis	1 ,,
Tetanus	1 ,,
18 causes. Total, 55[a] fatal cases.	

[a] In this number are included 5 cases which died after amputation had been performed.

"Of these 238 cases amputation of the limb was performed at various periods after excision of the knee-joint on thirty occasions, on account of a variety of circumstances. Of these thirty amputations, which were all through the thigh, twenty-five recovered, and five died.

"The circumstances which necessitated amputation have already been mentioned when treating of the various cases which deserved independent remarks; but they may briefly be divided into main classes—viz., those in which amputation was performed during the time of treatment, *i.e.*, during the first four to six months after excision, and those in which amputation was not deemed advisable or imperative till a much later period. Thus, for instance, Mr. Benfield amputated almost immediately after excision in his solitary case; Mr. Birkett on the thirty-eighth day; and myself not till two years and three months after excision of the joint.

"The definite per-centage of deaths in these various cases may be obtained as follows:—

"In the whole number of cases—291—the operation of excision terminated fatally in seventy eight instances, showing a proportion of one death in 3·7 cases.

"In the more limited number of cases—238—performed by British surgeons, of which I have the fullest record, there occurred fifty-five deaths, showing a proportion of one death in 4·3 cases.

"In these calculations I have included the five cases which sank after amputation had been performed when excision had failed.

"The proportion of deaths after amputation in the thirty operations is, one death in six cases."

Mr. Price analyzes the conditions of disease, which may be submitted to excision, in a masterly manner, and then proceeds to "discuss the circumstances which by some are said to preclude the performance of excision." The age of the patient is here more especially referred to, and in spite of Mr. Price's evident leaning in favour of excision, we are irresistibly led to conclude, even from his own remarks, that excision in the young, even where the line of epiphysary junction is spared, gives a result that no surgeon need care to boast of. Mr. Pemberton and Mr. Syme have set this question at rest as to the limb ceasing to develop in length. Mr. Keith's case, much quoted to prove the contrary, fails to do so, now that the boy has grown up. At fifteen years of age his limb was seven inches shorter than the other, and is described as a mere appendage. All that can be said is, that amputation may leave matters even worse in some cases. Either alternative is devoutly to be avoided if possible by any skill and patience; and, undoubtedly, if the surgeon have the opportunity to excercise them he will

seldom be called upon to decide upon the question of amputation or excision in the child. Their powers of recovery in favourable circumstances are perfectly marvellous.

We must now take leave of a book, which must be for all time one of the authorities on its subject—imperfect, and in some respects elementary, as it is, and must, unfortunately for us, continue. Many thanks are due to Mr. Henry Smith for his share in its production. His notes are most valuable, and often supply a gap in the original. The illustrations are perfect, and make us regret that circumstances prevented the publishing of all that are referred to in the text. Opposite the title page lies the shadow of the accomplished author looking upon us with a calm reflecting eye, and already foreshadowing, in the pale cheek and wasted hand, the early doom which has been to surgery so great a loss.

A Practical Treatise on Urinary and Renal Diseases, including Urinary Deposits. Illustrated by Numerous Cases and Engravings. By William Roberts, M.D., F.R.C.P., Lond.; Phys. to the Manchester Royal Infirmary; Lecturer on Medicine in the Manchester School of Medicine. Pp. 517, 8vo. London: Walton and Maberly. 1865.

We have had much pleasure in presenting to our readers a notice of this book, which is the *bonâ fide* "work" of an English provincial physician of standing. Dr. Roberts divides his book into three parts. The first consists of four chapters, and deals with the physical and chemical properties of the urine in health and disease, as well as with urinary deposits. Chapter I. contains a summary of the properties and composition, the physiological and pathological variations of the urine; various methods of examining it; extraneous matters to be found in it; and the changes which occur in it by keeping. Chapter II. treats of the physical properties of the urine—its odour, colour, specific gravity, quantity, and reaction. It is, however, opened with the somewhat remarkable statement, that the natural odour of healthy urine "may be described as *urinous*." Decidedly so, we should say. On p. 20, Dr. Roberts observes that—"when the urine is unusually *scanty* it should be ascertained, before pronouncing it a morbid phenomenon, whether the patient has abstained from liquids above his habit,

whether water has been eliminated in excess by some other channel, as the skin or bowels." He then proceeds to remark that the urine is always scanty in cirrhosis of the liver, throughout the entire course of some forms of Bright's disease, and in the last stage of all forms of that affection; also in any condition of the heart which directly or indirectly causes passive congestion of the renal veins; in the collapse period of cholera, in the latter stages of all organic renal disease, and when any mechanical obstacle obstructs the flow of urine. Chapter III. treats of the chemical constituents of the urine, of their variations, and of inorganic deposits. In it Dr. Roberts not only gives clear and concise descriptions, but he also furnishes the reader with well executed engravings in illustration of this part of his subject. Chapter IV. treats of abnormal substances and organic deposits in the urine. Under this head we may select for a few remarks Section VI.—"Blood in Urine—Hematuria." After mentioning the mode in which an admixture of blood with the urine may be recognized by its colour, and debating the sources from which the blood may come, he very properly, in our opinion, affirms that—"the microscope is the surest means of discovering it;" and forthwith gives an enlarged engraving of blood corpuscles in the urine as they appear by the aid of that instrument. As a specimen of the concise manner in which he sets down the great variety of causes from which hemorrhage may arise we give the following classification from page 99:—

"1. *Local lesions.*—External injury, violent exercise, calculous concretions, ulcers, abscesses, cancer, tubercle, parasites, active or passive congestion, Bright's disease.

"2. *Symptomatic*—in purpura, scurvy, eruptive and continued fevers, intermittent fever, cholera, &c.; mental emotion.

"3. *Supplementary or Vicarious*—to menstruation, hemorrhoids, asthma."

In his directions for testing for albumen with nitric acid, on p. 118, he gives this very practical suggestion, the value of which we ourselves have fully appreciated:—Fill a test tube to the depth of about an inch, then, inclining the tube, pour in strong nitric acid in such a manner that it may trickle down along the sides of the tube to the bottom, and form a stratum some quarter of an inch thick below the urine. The advantage of this plan is that thus there is scarcely any mingling of the two fluids, while, if albumen be present, *three* strata or layers will be observed—"one, perfectly

colourless, of nitric acid at the bottom; immediately above this an opalescent zone of coagulated albumen, and a-top the unaltered urine. If there be only a trace of albumen, two or three minutes elapse before the opalescent zone becomes visible." Further on, at page 129, when treating of testing for sugar, Dr. Roberts remarks, with reference to Moore's test (boiling with liquor potassæ), that while it is a very convenient one, for many reasons, yet that it is wanting in delicacy, and is liable to a notable fallacy. It does not answer until the proportion of sugar rises to about 0·3 per cent, or one grain and a-half to the ounce.

Part II. discusses those urinary diseases of which the chief characteristic is an alteration of the urine. It comprises two chapters on Diabetes (I. Insipidus; II. Mellitus); Chapter III. on Gravel and Calculus; and Chapter IV. on Chylous Urine. This portion of the work is very practical; is replete with records of cases; with the results of Dr. Roberts' experience, and of the experience of others. We are, however, surprised to find that the entire series of observations on diabetes and other urinary conditions, long since published in this Journal by that indefatigable follower of medical science, the Rev. Professor Haughton, are not, so far as we can see, once named or referred to in any way, although Dr. Roberts professes to give, and, indeed, with this notable exception, does give a complete treatise on the subject, and a long list of references to other writers. It cannot be that Dr. Roberts is ignorant of the papers of which mention is now made; the want of allusions to them is a considerable literary defect in his book, which we would point out with the hope that it may be remedied in his next edition. At the same time, we must note another inaccuracy; the name of our distinguished fellow citizen, Dr. Robert M'Donnell, is given as "M'Connell," and his writings are quoted under that designation no less than four times on page 183. It is remarkable that these errors occur with regard to Irishmen. Also, on page 29, the lectures of Graves are quoted with seeming accuracy, but without referring to the particular edition referred to by Dr. Roberts.

Notwithstanding these errors, however, we cannot shut our eyes to the excellencies of this work, one special feature of which is that at the beginning of each chapter he gives the bibliography of the subject. Space will not allow us to give in full, as we should greatly like to do, the formula for making bran-cakes for patients affected with diabetes mellitus. It was supplied to Dr. Roberts by

Dr. Camplin, and will be found at length in a long foot note on page 190.

Part III.—the best part of the book, in our opinion—treats of organic diseases of the kidneys. Chapter I. deals with Congestion; Chapter II. with Bright's Disease, chiefly as to its General Etiology; Chapter III. is on Acute Bright's Disease; Chapter IV., on Chronic Bright's Disease; Chapter V., on Renal Suppuration and Embolism; Chapter VI., on Pyelitis and Pyonephrosis; Chapter VII., on Renal Concretions; Chapter VIII., on Hydronephrosis; Chapter IX., on Cysts and Cystic Degeneration; Chapter X., on Cancer; Chapter XI., on Benign Growths; Chapter XII., on Tubercle; Chapter XIII., on Entozoa; and Chapter XIV., concludes with an account of anomalies in the position, form, and number of the kidneys.

We naturally turn to the account of Bright's Disease, under which title we find classed cases characterized by albuminuria and dropsy, depending on structural changes in the kidneys. Here we see that Dr. Roberts treats the entire subject from a clinical rather than from an anatomical point of view. He classes all varities of Bright's disease under the two general heads "acute and chronic;" meaning by the former "a compact and universally recognized group, which formerly went under the designation of 'inflammatory dropsy.'" It corresponds to the acute desquamative nephritis of Johnson, to the first stage of Frerichs, and to the acute tubular disease of Dickinson. Three types of the chronic affection are recognized:—1. Cases which have lapsed from the acute state (kidney—smooth, white, generally large, exceptionally dwindled). 2. Cases which have been chronic from the beginning (kidney—granular, red, contracting). 3. Cases associated with waxy or lardaceous (so-called amyloid) degeneration of the kidneys.

Remarking that the abuse of spirituous liquors ranks probably higher than any other single circumstance as a determining cause of Bright's disease, while the use of mercury has *not* been found by wide observers to produce this effect; Dr. Roberts enters fully into the symptoms, varieties, and complications to be invariably or frequently met with, and illustrates his positions by records of well-selected cases. He specially notes the frequency of its complication with phthisis, having witnessed so many as eight cases within the last two years; declares the prognosis to be "exceedingly gloomy;" and is of opinion that in confirmed cases the treatment should aim at these objects:—"(*a*) To hinder the further extension

of the structural changes in the kidneys; (*b*) to prevent the occurrence of uremic and inflammatory accidents; and (*c*) to palliate or remove certain threatening or burdensome symptoms—anemia, dropsy, dyspeptic and uremic phenomena, &c." (p. 366).

The effects of embolism in the kidney (Dr. Roberts states) are of very slight clinical importance; they pass by, in the immense majority of cases, without recognition; their occurrence is always secondary to some much more grave primary disorder, which altogether dominates the prognosis and treatment. Sometimes the larger ones go on to gangrene, and, still more rarely, to suppuration." (p. 381).

Passing by a very interesting portion of this book—that on Pyelitis and Pyonephrosis (Chap. VI.)—we come to Chapter VII. on Concretions in the Kidneys. Here Dr. Roberts is very practical. Under the head of *treatment* we give the following specimen:—

"During the paroxysms of renal colic, the remedies indicated are:—warm baths, emollient enemata, cupping the loins, and, in highly sthenic cases, venesection. The dolorous spasm of the ureter must be combated by free administration of opium. This drug is freely tolerated in cases of this class, and full doses should be repeated until the system is plainly brought under its influence. When the irritability of the stomach is such as to prevent the absorption of the drug, it should be introduced per rectum or by subcutaneous injection. Belladonna may be substituted were opium disagrees. The secretion of the urine should be encouraged by warm demulcent drinks; hot poultices should be applied to the loins or abdomen, as the local symptoms indicate."

He further observes that change in the position of the patient sometimes suffices to dislodge a calculus which lies upon, but has not become fully engaged in, the orifice of the ureter, while manipulation of the abdomen in the course of the ureters also facilitates the descent of the concretion. "Dr. Simpson witnessed relief follow complete inversion of the body."

Chapter VIII. discusses the morbid anatomy and etiology of hydronephrosis; Chapter IX. treats of cysts found in the kidneys under four practically different circumstances:—1. Scattered cysts in kidneys otherwise healthy; 2, disseminated cysts in the atrophic form of Bright's disease; 3, congential cystic degeneration; 4, general cystic degeneration in adults. Chapter X., treating of cancer, states that the species of it almost invariably found in the kidney is the encephaloid (fungus hæmatodes). Several cases in

point are detailed; and one of them, of which an engraving is given, is specially worthy of note. It occurred in the Middlesex Hospital, in a girl aged six; and the tumour covered the entire front of the body from the mammary region down to and resting on the knees when in the sitting posture. In this case no cancerous deposit was found in any of the other viscera.

Passing over the discussion of "Benign Growths," in Chapter XI., we come to "Tubercle of the Kidney," in Chapter XII. This is treated of under the heads of *primary* and *secondary;* the former is the real local disease, the secondary being only a part manifestation of tuberculosis co-existing in other parts of they body. The chapter on "Entozoa in the Kidneys" (Chap. XIII.) is particularly deserving of perusal—enriched, as it is, with curious cases and numerous engravings. The parasitic worms which infest this locality are—*ecchinococcus hominis*, or *hydatid*, *bilharzia hematobia*, *pentastoma denticulatum*, and *strongylus gigas.* The two last are extremely rare; the second occurs in Egypt, at the Cape of Good Hope, and certain other hot countries, while the first is by far the most common in these latitudes. Chapter XIV. concludes this work by giving an account of anomalies as regards position, form, and number. Thus we have fixed malposition of the kidney, movable kidneys, the horse-shoe kidney, and the solitary kidney, each in turn dilated on.

We have now endeavoured to give a short outline of the character and contents of Dr. Roberts' book, which we gladly hail as a valuable addition to medical literature. As a comprehensive treatise on the subject with which it deals we cannot but look on it as the best book yet published in our language; its copious references cannot fail to give it a permanent rank in the libraries of the medical scholar, while its eminently practical, and, therefore, highly useful character, will certainly enhance its value in the eyes of the busy practitioner.

On Some of the Causes and Effects of Valvular Disease of the Heart; being the Croonian Lectures of the Royal College of Physicians for 1865. By THOMAS B. PEACOCK, M.D., F.R.C.P.; Physician to St. Thomas' Hospital, &c. Pp. 114, 8vo. London: Churchill, 1865.

ANYTHING from the pen of Dr. Peacock is likely to be worth reading. With this impression we sat down to peruse his three lectures; and we confess we have not been disappointed. Whether we regard the style, the division of the subject, or the subject-matter, the book before us is worthy of perusal. The cases given are numerous, and well selected; showing that Dr. Peacock has had that amount of practical experience which fairly warrants him in setting his conclusions before the profession. Illustrations by means of engravings are here and there met with; and we feel bound to say that they render the meaning of the lecturer much clearer than it otherwise would be.

The first lecture is on "Diseases originating in Malformation;" and on page 2 Dr. Peacock thus tabulates the causes of valvular disease or defect.:—

"1. Malformation of valves, arterial and auriculo-ventricular,	Giving rise to	Regurgitation, Obstruction, or Obstruction and Regurgitation.
2. Injuries of valves, arterial and auriculo-ventricular; immediate and gradual,	"	Regurgitation, with or without Obstruction.
3. Alterations in capacity of orifices and cavities,	"	Regurgitation from erosion or mal-adjustment.
4. Inflammatory affections, chiefly rheumatic, acute, and chronic.	"	Obstruction, Regurgitation, or Obstruction and Regurgitation."

Of these different causes of disease Dr. Peacock treats only the first three; observing that "the fourth has been so fully illustrated as neither to require nor admit of further elucidation" on his part.

The malformation, or irregular development, may appear (according to Dr. Peacock) where the valves are in excess; or where they are defective. In the former case, when there are only two semi-lunar valves, at either of the two arterial orifices, and one of

these curtains is considerably larger than the other, the larger curtain not being properly supported becomes stretched, falls below the level of the other segments, incompletely closes the orifice during the diastole of the ventricle, and so permits regurgitation. In the latter case, where the whole of the curtains are blended together and a kind of septum, perforated by a larger or smaller opening, is stretched across the aperture, more or less obstruction is necessarily occasioned. Eleven cases in point are now given. Some are cases of disease of the arterial valves; while others are cases of disease of the auriculo-ventricular valves; these cases extend over several pages, and the first lecture is concluded with remarks on other defects in the conformation of the heart or arteries which may occasion disease, such as atrophy of the valves, and want of just proportion between the size of the ventricles and the capacity of the orifices and vessels leading from them.

Lecture II. is on diseases originating in injuries, and in alterations in the capacity of the orifices and cavities of the heart. Here Dr. Peacock remarks that the valves may be indirectly injured in external accidents; but that they more generally give way under violent muscular exertion; also that these injuries may be immediate or gradual. Notes of five cases are added in support of these positions; and in summing up his remarks Dr. Peacock says:—"It has long been observed how frequently disease of the heart, and especially of the aortic valves, occurs in persons who follow laborious occupations, as smiths, strikers, and riveters, bricklayers' labourers, hodmen, &c. I have seen several cases of incompetency of the aortic valves in the 'along-shore men,' as they are called—the men who are employed to load and unload the small coasting vessels at the wharves on the banks of the Thames, the coal-whippers, and the Newgate Market and Deal porters—men who carry very heavy weights upon their shoulders, and often up slanting planks or landings, in which positions the stress upon the aortic valves must be excessive. When in Cornwall, two years ago, I met with a case of incompetency of the aortic valves in a man employed in a somewhat similar way. He was a 'coacher,' or one of the men who push the trucks containing the rock or ore along the tramways at the bottom of the mines—an operation performed in a stooping position, and which requires great force. Many of these occupations are commenced at early periods of life, and must be especially injurious to persons who have not obtained the full vigour. I have frequently known cardiac symptoms occur

under these circumstances in young persons employed in laborious occupations, and not unfrequently in girls engaged as servants or nurse-maids, before they had attained their full strength. In some of the cases there is no reason to suppose that the patients have ever had any inflammatory symptoms, and the cardiac affections are apparently due to overstraining of the valves or to some injury of the muscular structure of the heart."

Dr. Peacock proceeds with a most elaborate discussion on some of the causes of valvular defect; gives a lengthened summary which is too long to insert here; even an analysis of that summary extends over several pages, and is accompanied by a table of 63 cases of valvular disease and defect. We may merely remark that of 26 cases of aortic valvular disease, 9 or 34·6 per cent. are referred to malformation; 2 or 7·4 to injury; 6 or 23·07 to rheumatism and other combined causes; and 9 or 34·6 to various other combined causes.

Of 17 cases of uncombined mitral disease 11 or 66·7 per cent. were traced to rheumatism; and 6 or 25·2 per cent. to other causes; while of 16 cases of combined aortic and mitral valvular disease 2 or 12·5 per cent. were connected with malformation; 10 or 62·5 were referred to rheumatism and other causes; and 4 or 25· per cent. were also referred to other causes. "Of the whole 33 cases, 21 may be regarded as originating in rheumatism, or 63·6 per cent."

Lecture III. discusses some of the *effects* of diseases of the heart. Tables are given of the weight and size of the healthy heart in the two sexes, at different ages, and in persons dying from acute and chronic diseases; of the weight of the heart in different forms of disease, and when variously diseased; and of the dimensions of the heart in different valvular and other forms of disease. Dr. Peacock then makes inferences as to the weight and dimensions of that organ in different diseases; and enlarges on the symptoms and results of different forms of valvular disease. Finally, he winds up by announcing his general principles of treatment. His observations on the use and abuse of digitalis are particularly valuable, combating, as they do, some cherished opinions regarding the omnipotence of that medicine in some forms of cardiac disease. Regarding it, and other depressing remedies, he quotes the words of Dr. Stokes, "that the practitioner should never forget that local diseases, themselves incurable, may co-exist with an excellent state of the general health, for a period indefinitely long; and the

conclusion is but too obvious, that, as the disease cannot be cured, the system at large should not be tampered with." Dr. Peacock concludes with the cheerful opinion, that, we may "in many cases enable the patient to enjoy a large amount of health and vigour for many years, and in some instances probably for the full period of human life."

We look on this book as a very valuable addition to medicine and cardiac pathology; and we have much pleasure in commending it to the profession.

A Theoretical Inquiry into the Physical Cause of Epidemic Diseases, accompanied with Tables. By ALEXANDER HAMILTON HOWE, M.D., Honourable East India Company's Service. Pp. 171. London: John Churchill.

THE books upon plagues and epidemics threaten to become a plague of themselves, as the works upon these subjects, published during the past twelve months, are legion.

It will be seen, from the title of Dr. Howe's book, that he admits but *one* cause for all epidemics no matter what disease may assume the epidemic form.

Although Dr. Howe makes an apology for the irregularity in his chronological arrangement of recorded epidemics, we can scarcely accept his apology as in any way excusing him for sending out such an ill-arranged table as that which composes more than half of his book.

Dr. Howe has evidently spent a great amount of time and trouble in collecting his information, and must have compiled his notes with considerable care, as we think he has scarcely omitted any epidemic of consequence from his list. We regret we cannot regard his list of recorded epidemics at all in the light of a chronology of those events, inasmuch as it is arranged without any regard to the order of time in which they occurred; he also, by the careless huddling together of his notes, deprives us of the means of rapidly and carefully testing the truth of his statement, that epidemics occur at intervals of about eighteen years.

The main point, however, in Dr. Howe's work is, that all epidemics are to be ascribed to lunar influence. The lunar influence which he considers of chief importance, is the revolution

of the lunar node, combined with a double revolution of the lunar apse-line. He deduces from his list of epidemics, that these visitations occur at intervals of eighteen years, the variations being from seventeen to twenty years. The author states that the revolution of the lunar node is the only natural phenomenon corresponding with this period of eighteen years. Has Dr. Howe ever considered the effect of the variations in the magnetic condition of the earth, which perhaps might answer as well, granting the same margin as Dr. Howe demands for his eighteen-years period? Dr. Howe considers that the variations in the distance of the moon from the earth (24,000 miles) during its nodal revolution, must have considerable influence on the state of the earth's atmosphere, although such has never been demonstrated by science. There is no doubt, such may be the case, but on the other hand, we have the monthly variations in all states affected by lunar influence, and yet it is now well known that these variations have no influence on the prevalence of disease, although at one time such was supposed to be the case. We think that if the nodal changes produce such powerful effects, the monthly variation should have some *slight* influence.

To prove that epidemics do *really* follow definite periods in their visits, it should be shown that they follow this rule in each and every locality at every visit of each disease. We think, Dr. Howe has utterly failed to show a sufficient number of examples to prove his theory. One epidemic in China now, and another in America eighteen years hence, prove nothing as to periodicity of cause, although such may happen in a dozen scattered localities over the earth, through a succession of periods of eighteen years. Dr. Howe cites cholera as an instance of a disease following, in its progress, the direction of the regression of the lunar node along the ecliptic; to show that the direction of the regression of the lunar node guides the direction of the spread of cholera it is necessary that it should follow the same rate as the regression of the node, which it does not.

In conclusion, we regret that Dr. Howe did not carry his diligence a little further, and arrange his list so as to make it a chronological table of epidemics; if he had done so, his book would have been of considerable value for reference, independent of the wild theories it contains, which the author states he was in such a hurry to get before the public that he had no time to arrange the only valuable part of the work.

A Manual of Practical Hygiene, Prepared Especially for Use in the Medical Service of the Army. By EDWARD C. PARKES, M.D., F.R.S.; Professor of Military Hygiene in the Army Medical School; Member of the Medical Council of General Education; Examiner in Medicine in the University of London; Emeritus Professor of Clinical Medicine in University College, London. London: John Churchill and Sons, 1864; pp. 612.

IT will be in the recollection of many of our readers that in 1857 a Royal Commission was appointed to inquire into the sanitary condition of the army in England. As a result of the labours of this Commission, a new edition of the *Queen's Hospital Regulations* was published by authority in 1859. The position of the army medical officer was entirely altered by the new regulations. Up to this time, although frequently consulted as to the preservation of health amongst troops and the prevention of disease, the army surgeon was, in strictness, merely officially entrusted with the care of the sick. The regulations of 1859 gave him a new position. He is ordered "to advise commanding officers in all matters concerning the health of troops, whether as regards garrisons, stations, camps, and barracks—or diet, clothing, drill, duties, or exercises."—p. 7.

Another recommendation of this Commission was that a medical school should be established, in which "the specialities of military medicine, surgery, hygiene, and sanitary science" should be taught to the young medical officers about entering the army.

Dr. Parkes' work, now before us, has for its object to carry out the wishes of the Commissioners as regards sanitary science, by providing a text-book of hygiene, illustrated by examples drawn from army life, for the gentlemen attending the army medical school.

It is true, that in writing this work the author has had to deal with but one sex, a certain age, and a particular calling; he conceives, however, that he is entitled to designate his work as one on general hygiene, as the principles of hygiene are tolerably fully discussed. We conceive that he is indeed justified in assuming the general title, and we assure our readers that they will find in the pages of Dr. Parkes' book not only much that is instructive and entertaining, but a great deal of information quite as important to the civil as the military medical man. The work is divided into

two books. In the first, the chief subjects of hygiene—water, air, food, sewerage, clothing, climate, &c.—are arranged in a convenient order, and discussed in a very clear and masterly style. Some other subjects, such as meteorology, statistics, physical training, are introduced, which it is of consequence that medical officers should be familiar with.

These chapters contain much that is useful, not only to the army surgeon, but to all professional persons who have not books on these topics at hand. The book is indeed valuable as a work of reference, and contains a great deal compressed into a small space.

The second book is more special in its character, and consequently less important in the eyes of the civilian medical man; it is devoted to the service of the soldier. The first chapter is given to the recruit—his weight, height, age, training, &c.; the remaining chapters to home and foreign service, service on ship-board, and service in war.

When speaking of foreign service the author mentions many important particulars touching each station garrisoned with British troops. Thus of Malta he says:—

"MALTA.

"Size 17 miles by 8. Usual peace garrison=6000 to 7000; period of service, three years; population (civil) in 1851=98,021.

"*Geology.*—Soft, porous rock; the greater part is sandstone, resting on limestone; in some parts marl and coral limestone over the sandstone. In the centre of the island, at Citta-Vecchia, there is, in order from the surface, alluvium, upper limestone, red sand, marl, sandstone, and lower limestone. Valetta is on thin alluvium, with thick sandstone below, and beneath this the lower limestone.

"*Climate (at Valetta).*—Mean of year, 68°; hottest month (July), 77°; coldest (January), 57°; amplitude of the yearly fluctuation, 20°; extreme yearly range (from highest to lowest temperature in shade), 62°; from 100° in July to 39° in January; mean yearly range, about 50°; extreme monthly range (*i.e.*, from highest to lowest in month), about 25° to 35°.

"Undulations of temperature are frequent, and there are often cold winds in winter from N.W. The south-east wind is an oppressive sirocco, raising the temperature to 94° or 95°. It is chiefly in the autumn, and blows for from 70 to 80 days every year. At Citta-Vecchia (600 feet above the sea) the temperature is lower and the air keener. Rain-fall about 32 inches. Chief rain in November, December, and January; less in February and March; small in amount in the other months. From June to August almost rainless.

"*Humidity.*—(Mean of 1859-60); observations at 9.30 A.M. and 3.30 P.M.

	Dew-point.	Grains of Vapour in a cubic foot.	Relative Humidity.
Mean of year,	60·5	5·86	62
Highest in year (August),	72·7	8·73	...
Lowest in year (February),	49	3·96	...

"Malta thus appears to be a dry climate, *i.e.*, with little relative humidity.

"*Sanitary Condition.*

"Much has been done of late years, and, as far as external cleanliness goes, Valetta is very clean. Water supply from rain and springs (the largest of which is in the centre of the island, and the waters of which are led by aqueduct), is not very deficient in quantity (8 to 10 gallons per head), and, except in some places, good in quality, though the rain-water contains chlorides from the spray falling on the roofs of buildings. Some of the tanks are too near the sea, which percolates into them. The tanks require, however careful looking after. Within the lines there are 272 public and military tanks, with storage for 55 millions of gallons, and 4,294 private tanks, with storage for 323 millions of gallons. The military tanks, if full, would give 6 gallons of water per man daily for eleven months, but even now the water often falls short. The water is now carried everywhere by hand, and the drinking-water for the men is not filtered. The sewers in Valetta are bad in construction and outlet, and much typhoid has been and is still caused in consequence. In many cases 'they are nothing but long cesspools.'[a]

"The barracks are bad, many casemates being used, and buildings intended for stores, and not for habitations. In some cases all sanitary considerations have been sacrificed for the purposes of defence. They are built of soft sandstone, which both crumbles and absorbs wet. The ventilation of the casemates is very bad. The Barrack Commissioners, in their Report, recommended that in every way which can be done the ventilation should be improved by admitting the wind, especially from the north, and that each barrack will require a separate plan to meet the particular case. They recommend that air-shafts shall be made much larger than ordered for home service, viz., 1 square inch for every 20 cubic feet of space, or for a barrack of twelve men with regulation space (7200÷20=) 360 square inches (=$2\frac{1}{2}$ square feet) of outlet opening. At the present time the amount of cubic space is below the home service amount (600 cubic feet) and the superficial area is very small, one-fourth of the men having less than 40 square feet each.

[a] Barrack Commissioners' Report, p. 111.

"Means for ablution are very deficient. Urinals and water latrines are made of porous stone, and are also bad in construction.

"It is therefore evident that the condition of Malta is a parallel to that of Gibraltar, and very much the same diseases may be expected, viz., typhoid fever from bad drainage, and lung disease from the faulty ventilation. As the water is less impure, the amount of dysentery may be expected to be less.

. . . . . .

"In time of war, the dangers at Malta would be the same as at Gibraltar; the aqueducts might be cut by a besieging force, and the water supply restricted to the tanks. Although these are supposed to hold a large quantity, they are not kept full, and could not, perhaps, be rapidly filled. The garrison might be driven to distil the sea water. The Barrack Commissioners very properly strongly advise that a tank inspector should be appointed. A still more serious danger would be the overcrowding of a war garrison. Doubtless, in case of a war, the garrison would only be concentered in the lines when the siege commenced, but the crowding during a siege of three or six months might be very disastrous. This danger should be provided for beforehand by a clear recognition of what accommodation would be wanted for war, and how it is to be obtained without violating either the conditions of health or of defence."

We are happy to learn from Dr. Parkes that the Director-General has recommended, and Lord de Grey sanctioned, the issue of a box containing sufficient apparatus and reagents to enable medical officers to perform chemical processes required in the analyses of water and air, and in the examination of food.

We can indeed say that we have derived much pleasure and instruction from perusing Dr. Parkes' book, and we look upon it as a work not only likely to improve materially the position of the British soldier, but as one which may be consulted with profit by any person interested in the great questions of sanitary science and the prevention of disease.

Tetanus.—Eine Physiologische Studie. Von DR. JOHANNES RANKE, Privatdocent der Physiologie und Assistent am Physiologishen Institute in München. Leipzig: Verlag von Wilhelm Engelmann, 1865, pp. 468.

Tetanus.—A Physiological Study. By DR. JOHANNES RANKE, Lecturer on Physiology and Assistant in the Physiological Institute in Munich. Leipzig: Engelmann, 1865, pp. 465.

THE title of the book before us requires explanation: it is a physiological inquiry into the circumstances attendant on muscular contraction. It is, in fact, a work on the physiology and physiological chemistry of muscle and muscular action.

It is a work of great merit, and one which places its author in the foremost rank among the physiologists of Europe; it gives evidence at once of that careful research which characterizes German investigation, and of genuine originality.

In a review like the present we can only lay before our readers an epitome of the contents; but we strongly recommend those interested in the important subject treated of in Dr. Ranke's book to peruse it for themselves.

Dr. Ranke commences by giving a brief history of the chemistry of muscular action up to the time when he commenced his researches. In this introduction he reminds us that one of the chief problems for physiological investigation has always appeared to be the solution of the question as to the cause by which muscular action is brought about.

There was a time when the consideration of this and similar problems was entered upon with *à priori* conjectures—with philosophic systematic speculations. Many groundless hypotheses thus took their origin.

F. Magendie, at the time when he strove to introduce an exact method of physical investigation into physiology, spoke of the condition of this question briefly and with candour as follows:—"What changes take place in the constitution of the muscles during the state of contraction? We do not know; and in this respect muscular contraction does not differ from the vital processes, of which no explanation whatever can be given."[a]

Before the researches of Liebig concerning flesh meat no

[a] Grundriss der Physiologie Translated by Heusinger, 1820.

chemistry of muscle existed; how could there be then a chemistry of muscular tetanus? The first commencement of any such is found in the researches of Liebig just mentioned.[a]

The following is a sketch of the various amount of kreatine in different kinds of flesh:—

The order in which Liebig arranges the flesh of mammalia according to the amount of kreatine in each, viz., so that that flesh which is richest in kreatine takes the first place, and that containing the least the last, appears to be the same in which they must be arranged with reference to the amount of motion which these animals are in a position to perform during life in relation to the mass of their muscles.

The order is as follows:—The martin, horse, fox, roebuck, stag, hare, ox, sheep, pig, calf.

At page 36 of the work already quoted Liebig says:—

"The difference in the amount of kreatine is striking, even in the same class of animals. The flesh of a fox which had been fed for two hundred days upon flesh meat afforded not a tenth part as much kreatine as was obtained from an equal weight of the flesh of foxes killed in hunting.

"The quantity of kreatine in the muscles of an animal is in an obvious relation to the amount of fat or to the causes which determine the putting up of fat. From fat flesh mere traces only are often obtained, but always far less kreatine comes from lean, the quantity of fibre being equal. The fed fox already mentioned yielded over one pound of abdominal fat, whilst the fat of coursed or hunted foxes was scarcely appreciable by the eye."

In the heart of the ox kreatine is present in considerable quantity, although otherwise ox-flesh takes a low place in the range of those containing kreatine.

The flesh of animals which are in a condition to perform more muscular work in proportion to the bulk of their muscles, contain more kreatine.

By means of repose—the cause of an accumulation of fat—the quantity of kreatine in the flesh diminishes. By means of coursing and hunting—muscular work—it increases.

The most actively-employed muscle of the organism—the heart—

[a] Chemische untersuchung über das Fleisch und Leine Zubereitung zum Nahrungsmittel. Heidelberg, 1847, C. F. Winter. Ann. Ch. Pharm., lxii., 257.

contains a greater quantity of kreatine than the less actively-exerted muscles of the trunk.

It is clear that these observations must be thus explained:—

Muscular contraction is connected with an increase of the kreatine in muscles.

There was proof that during tetanus appreciable chemical changes went on in muscle. Thereupon a foundation was gained for assuming that one would be in a position through further researches in this direction, to find a physical groundwork for the production of muscle force in the forces set free in its disintegration and oxydation.

The kreatine could only be looked upon as the product of the disintegration of the muscular substance.

The path of direct research concerning muscular action, which Liebig had opened with this first result of his enquiry, was not the only one which was mentioned by him with the object of establishing a chemistry of muscular tetanus. He had already, in his *Thier-Chemie*, page 202 ff, bestowed upon physiology two other methods, viz., the method of the comparative determination of urea and the method of determining the carbonic acid of respiration, with the same object.

When Liebig came to the conclusion that all the nitrogen resulting from the nitrogenous material disintegrated in the body makes its appearance in the urine, he thereby gave the means of inquiring how far the work done by muscle is connected with the consumption of muscular tissue, which must yield nitrogenous products as the result of disintegration. Indeed, this very method of investigation must appear all the more inviting as one must admit that changes, which may easily escape detection in a tissue so difficult to investigate as that of muscle, may be made out without much difficulty by the investigation of the urine after the comparatively easy chemical method of Liebig.

Besides that Liebig, in his theoretical observations on his experiments on animals, had laid much stress on the connexion between the exhalation of carbonic acid and the interchange of matter, he had also pointed out the method of investigation of the amount of carbonic acid separated during respiration as a means of determining the influence of movement in general on the interchange of matter of the organism; and thereby, secondarily, of that in the organs of movement, viz., muscle.

By creating this method of inquiry he opened up the question as

to the influence of muscular movement on the chemical constitution of muscle, and along with that the whole organism in the true sense of the word. We shall see that this question was approached from different sides promptly, and indeed, in some instances, too precipitately, for the accurate determination of the replies that were demanded.

All the three above-mentioned methods of investigation which Liebig had pointed out were forthwith tried to be turned to account by different investigators of the chemistry of tetanus.

C. G. Lehmann (Wagner's *Hdbch. d. Phys.*, Band II., s. 21; and Lehmann, *Phys. Chemie* Bd. I., s. 164, 1844) first of all gave his attention to the determination of the influence which muscular motion possesses over the elimination of nitrogen by the urine, viz., in the excretion of urea.

He came to the conclusion that active bodily exercise gives rise to an increase of the urea excreted.

The diet being nearly the same, he eliminated, as a general rule, 32 grammes of urea during a day of repose; but after considerable bodily fatigue, 36·37 grammes.

The researches of J. F. Simon (*Hdbch. d. angew. Med. Chem..*, 1842, II., 368) gave also an increase of urea excretion in consequence of persevering exercise, In this case also there was not given a complete determination of the relation as to diet.

The question then seemed settled; and what had been looked on as a reasonable postulate appeared to become a stronghold of physiology.

The discovery made by E. du Bois-Reymond and G. V. Liebig (*fils*) (in prosecution of the researches of Humboldt, Fontana, Tiedemann, and others) on the influence of the chemical composition of the atmosphere on muscular movement, especially the movement of the heart, strongly supported the idea of profiting by the methods of estimating the carbonic acid separated by respiration, as well as in connexion with the relative interchange of it in the muscle itself. This discovery was that the muscle of the frog, so long as it still reacts with electricity, absorbs oxygen and gives off carbonic acid, and that a muscle placed in nitrogen exhales carbonic acid until the extinction of its capability of being made to contract.[a]

Matteucci also made this observation, and showed distinctly that

[a] Müller's Archiv., 1850. 393; Berl. Acad. Ber., 1850, 339; Pharm. Centr., 1850, 769; Instit., 1851, 118.

an increase of the separation of the carbonic acid of muscle takes place in consequence of electrical tetanus.[a]

Valentin's researches corroborated the results already known; he further found that the muscle, after the cessation of its irritability, gives off carbonic acid and takes up oxygen.[b]

The researches as to the total amount of carbonic acid given off in respiration under the influence of exercise were for the most part carried on by Scharling[c] and Vierordt.[d]

They both found that after violent exercise the exhalation of carbonic acid seemed absolutely increased.

Prout (*Schweiggers Journ.*, XV., 60), on the contrary, found at the commencement of moderate exercise an increase of the carbonic eliminated in respiration, while on the other hand it was diminished by fatigue: even at the very first violent bodily exercise caused a diminution of it.

Séguin[e] had, moreover, already shown that during strong muscular movement more oxygen is consumed than during weaker ones.

More recent experiment verified the observation of the increase of carbonic acid under the influence of muscular movement.

The path also of direct chemical research concerning muscular tissue under the influence of tetanus was discovered, and gave some important results. E. du Bois-Reymond showed, that in consequence of the action of muscle an acid reaction appeared in it, whilst muscle during repose gave a neutral reaction. According to Liebig's researches—which, however, confirmed the fact as to the neutral reaction of fresh muscle—the acid existing in the muscle juice proved to be lactic acid. The phenomenon, therefore, observed by E. du Bois-Reymond of the juice of muscle becoming acid in consequence of tetanus rests on the formation of lactic acid from other elements of muscle.

Helmholtz[f] made the discovery that the fixed substances held in solution in the fluid expressed from flesh, which were extracted from muscle tetanised to exhaustion, were different from those

[a] Comptes rend. xlii., 648 ff.

[b] Archiv. f. Phys., Heilkunde, xiv., 431 ff., 1857.

[c] Berzelius Jahresbincht, xxiii., 602; xxvi., 829. J. pr. chm., xlviii., 435. Ann. ch. pharm., xlv., 214 ff.

[d] Archiv. f. Phys., Heilkunde iii., 536 ff. Physiologie des Athmens. Karlsruhe, 1845. Respiration in Hwbch. d. Phys., 1844, 828 ff.

[e] Memoirs de l'Academie de Paris, 1790.

[f] Müller's Archiv., 1845.

which could be obtained from the similar muscle of the same animal which had been left in a condition of repose.

This was for the time the first direct proof that the contraction of muscle was actually accompanied by a chemical change in the substance of muscle. The researches of Liebig regarding kreatine took place later. Helmholtz found that in muscle which had been tetanised the spirituous extractive matter was increased, while the matters soluble only in water were diminished.

The further discovery of Helmholtz[a] that muscle produced heat during contraction furnished proof that there was not only a disintegration of tissue during tetanisation, but that an increased oxydation takes place also.

Liebig's idea was, that all the nitrogen arising from the disintegration of the nitrogenous tissue of the animal organism was eliminated by the urine. It was in this way, as already mentioned, that the analysis of the urine became a means of determining quantitatively probable fluctuations in the amount of disintegration of the nitrogenous structures of the body, especially of the muscles, as might be expected to result from tetanus. We saw that the researches of Lehmann and Simon, which have been already mentioned, have been based upon this idea of Liebig's.

It is obvious, that if the urea were no measure of the change of tissue, in the sense supposed by Liebig—that if, besides, the separation of nitrogen through the kidneys, another elimination of nitrogen, not capable of being estimated, took place through some other channel—then the observations of Lehmann and Simon would prove nothing at all regarding the chemistry of muscular tetanus, even although correct as regards other considerations. The increase described by the above-mentioned writers in the elimination of nitrogen in the urine in consequence of augmented muscular movement was really considered as depending upon the increase of the interchange (wear and tear) in the work-doing nitrogenous organs, viz., the muscles.

If the proposition of Liebig were not tenable, then this specious argument would be good for nothing; muscular movement, indeed, might be able to change the relation of the actual separation of nitrogen in a gaseous form to the separation of it through the urine, somewhat to the advantage of the latter. Even although the foregoing observations were quite trustworthy, it would not on

[a] Müller's Archiv., 1848.

that account justify any assumption as to an increase in the interchange (wear and tear) of the nitrogenous structures of the body through the occurrence of tetanus.

The proposition that the urea was the measure of the disintegration of tissue seemed actually to totter.

Experiments upon diet, apparently conducted with accuracy, always gave a deficit in the elimination of nitrogen through the channel of the kidneys compared with the nitrogen taken in with the food.

The observations of Barral,[a] made upon himself and those of Th. L. W. Bischoff,[b] appeared the most exact. Both these authors found a deficiency of nitrogen. So likewise did Bidder and Schmidt.[c] For a cat an equal quantity seems to have been proved; but this observation stands quite alone.

In addition to this Regnault and Reiset,[d] in their researches on the respiration of animals, had recorded in many cases an exhalation of nitrogen during respiration corresponding to the deficiency of nitrogen in the urine, whilst in other cases, on the contrary, there was an absorption of gaseous nitrogen throughout the organism.

Matteucci and Valentin, in the experiments already alluded to on the carbonic acid given off from the muscles, had attempted to show that a separation of nitrogen also took place; the latter, however, conceived that in this observation an error had occurred in the experiment.

In the researches of Valentin regarding respiration, we find a heading, "Excess of Nitrogen and Error of Observation."[e] The excess of nitrogen is assumed to be supplied through the respiration.

If these observations, of which we only quote the more important, should be verified, then the method of determining the nitrogen in the urine as a confirmation of the examination of the amount of disintegration of the nitrogenous tissues of the body would fall to the ground.

Meanwhile, the observations of Lehmann and Simon got

[a] Ann. Ch. Phys. (3), xxv., 129; J. pr. Chem., xlviii, 257. The conclusions, also: Compt. Rend., xxvii., 361; and Pharm. Centr., 1848, 929.

[b] Harnstoff als mass de stoffwechsels., 1853.

[c] Die verdauungssäfte und der stoffwechsel, 1852.

[d] Ann. Ch. Phys. (3), xxvi., 299; Ann. Ch. Pharm., lxxiii., 92, 129, 257. The results: J. Pharm. (3), xvi., 297; Pharm. Centr., 1849, 683.

[e] Die Einflüsse der vagusläbmung, &c., 1857.

additional support from those of H. Beigel,[a] and W. Hammond,[b] and later from Genth[c] also.

These researches were, however, all rendered valueless so long as no proof could be adduced that no nitrogen was eliminated by the process of respiration.

Furthermore, on quite different grounds, all the foregoing researches concerning change of tissue, and the conclusions based on them, are worthless.

Before the experiments of Th. L. W. Bischoff and C. Voit[d] on diet, no experimenter had made really accurate researches in this direction. Everything which had hitherto been done concerning this subject was practically quite eclipsed by their extreme care.

In future similar investigations can only be carried on after their method.

They showed that the deficiency of nitrogen in the excreta so often asserted to exist did not exist in reality. In consequence of this Liebig's proposition was not so much reinstated on a basis of more exact enquiry as established as a new truth. The nitrogen in the excreta *is* a measure of the disintegration of the nitrogenous elements of the body.

C. Voit[e] undertook anew the researches of Lehmann. They proved before his accurate experimentive criticism not only to have been carried on on a faulty principle, but to be erroneous in their results also. C. Voit adduced proof that the increase of elimination of nitrogen (that is urea) which had hitherto been maintained to occur in consequence of muscular work, did not exist.

From all that had been hitherto gained nothing remained to serve as a basis for a mechanical explanation of the work done by muscle, except the results gained from muscle itself.

It is obvious that the observations of Helmholtz, of the increase of temperature in muscle during action, cannot be reckoned under this head. They show an increase in the oxydation of muscle in action, but nothing more. The forces set free in this case are not,

[a] Untersuchungen über den Harn und Harnstoffmengen, in den Verhandlungen d. k. Leopold. Akad. d. Naturf. Bd. 25 abthl. 1, 477, 1855.

[b] Americ. Journ., Jany., 1856.

[c] Untersuchungen über den Einflass des Wassertrinkens auf den Stoffwechsel. Wiesbaden, 1856.

[d] Die Gesetze der Ernährung de Fleischfressers, 1860.

[e] Untersuchungen uber den Einflass des Kochsalzes, des Kaffees und der muskelbewegung auf den stoffwechsel, 1860.

indeed, applied to the mechanical performance of the muscle, but to its increase of temperature.

There remained only, as established facts, the following, viz.:—

1st. In consequence of tetanus, muscular substance, which during repose has a neutral reaction, becomes acid.—Du Bois-Reymond.

2nd. Muscular work seems to increase the quantity of kreatine in muscle.—Liebig.

3rd. The relative proportions of extractive matters in muscle at rest and tetanus are different. In the latter there is found an increase of the alcohol extractives and a diminution of the water extractives.—Helmholtz.

4. Muscle during tetanus gives to the atmosphere more carbonic acid than when in a condition of repose.—Valentin and others.

This was the state of the question in 1860, when Ranke took up the subject.

Since then the most important facts added are the discovery of a true sugar in muscle by Meissner, and the confirmation by Sarokow of Liebig's observation as to the increase of kreatine in muscle in consequence of work accomplished.

We are now in a position to consider what are the results brought to light since 1860, by our author, and this we hope to do at a future period.

(*To be continued.*)

"*RECENT WORKS ON DISEASES OF THE LUNGS.*"

1. *Remarks on the Antecedents and Treatment of Consumption.* By Charles Drysdale, M.D., M.R.C.P. Lond.; F R.C.S. Eng.; Physician to the Farringdon Dispensary, London. M'Gowan & Danks, Great Windmill-street. Pp. 12.

2. *Consumption, as Engendered by Rebreathed Air and Consequent Arrest of the Unconsumed Carbonaceous Waste; its Prevention, and Possible Cure.* By Henry MacCormac, M.D. Second Edition, Enlarged and Revised. London: Longman, Green, Longman, Roberts and Green. 8vo, pp. 240. 1865.

3. *The Elements of Prognosis in Consumption, with Indications for the Prevention and Treatment.* By James Edward Pollock, M.D. London: Longman, Green, and Co. 8vo, pp. 432 1865.

4. *Australia for the Consumptive Invalid; The Voyage, Climate, and Prospects for Residence.* By Isaac Baker Brown, Jun. London: Hardwicke. Post 8vo, pp. 137. 1865.

5. *Winter Cough, Catarrh, Bronchitis, Emphysema, Asthma.* By Horace Dobell, M.D. London: Churchill and Sons. Pp. 182. 1866.

The first essay on our list was read before the Harveian Society last year; it is not very voluminous, but it contains a good deal of matter worthy of being perused and carefully reflected upon by others besides members of the medical profession.

Dr. Drysdale believes that in order to speak of the treatment of consumption, it is absolutely essential that we be well acquainted with the antecedents of the disease. If anything in medical science is certain, it is that if this frightful plague, from which mankind suffers so much, is ever to be staid, it is by looking to its antecedents, and endeavouring to remove the causes which give rise to to it, that alone we can hope to effect such an object. Hereditary transmission, intemperance, excessive tobacco-smoking, various personal antecedents, as sexual excess, disappointed love, topical influences, are spoken of by the author, but it is to his excellent remarks on poverty, as a cause of consumption, that we would especially direct attention.

"*Poverty.*—One of the most frequent antecedents of phthisis pulmonalis in Europe is poverty, or the inability to obtain the necessaries of existence by means of labour or otherwise. Lombard, D'Epiné, and Lebert have given sufficient evidence that this disease is much more prevalent among the poor than among the upper classes. This cause of consumption has not, in my opinion, been sufficiently dwelt upon by medical authors who have treated of the disease. The way, too, in which poverty, or the want of a sufficient supply of the essentials of nutrition, acts in producing consumption requires explanation. It would appear from Livingstone's account that the savage tribes of South Africa are subject to periodical want of food. In such cases the natives become exceedingly emaciated, and often die in great numbers of starvation. Nevertheless, this want of food does not develope in their hardy frames tubercles of the lungs, and we must, consequently, look to other causes beyond that of starvation to account for the large amount of disease in our civilized and industrial manner of existence. With us want of sufficient food is one of the most frequent antecedents of consumption. To recall this subject vividly before the mind I shall quote a few passages. The following is taken from one of

the daily London papers of the 26th October, 1864:—'An inquest was held at the Canrobert Tavern, Bethnal Green Road, last evening, on the death, from want, of Emma Jackson, aged forty years. Martha Burkhardt said the deceased used to bind two pair of boots a day, and she was paid 2½d. for one pair and 3d. for the second, which was of a better kind; she never earned 6d. a day, but she used to say she earned more, "so as to keep up the look of the thing." Dr. G. Rolf said there was disease of the lungs and heart; the *cause of death was the lung disease*, accelerated by exposure and want of the common necessaries of life.'

"In the sixth report of the medical officers of the Privy Council for 1863 there is a report by Dr. Edward Smith, 'On the Food of the Labouring Classes,' in which I find a table of the cost of food of various classes of our workpeople:—Silkworkers, 2s. 2½d. a week; needlewomen, 2s. 7d.; kid-glovers, 2s. 9d.; shoemakers, 2s. 7½d; stocking-weavers, 2s. 6¼d.: and Dr. Smith says:—1. No class under inquiry exhibited a high degree of health. 2. The least healthy are the kid-glovers, needlewomen, and Spitalfields weavers. 3. The average quantity of food was *too little for health and strength*. Of the needlewomen he says: 'This is the lowest paid class included in my inquiries; ordinary hours of work ten to twelve hours. The average income was only 3s. 11¼d. weekly per adult.' Mr. Godwin, in his work entitled 'Another Blow for Life,' says:—'Among the causes which lead to the evils we are deploring, we must not overlook the gradual increase of children, while in the case of the labouring man the income mostly remains the same. The want of *providence* on the part of these men is often commented on with good intentions; but, whilst admitting that better use might be made of their incomes in many instances, we must not omit to remember the cost of living in London, especially with a large family of children. As the children increase in numbers the wife is prevented from adding by her earnings to the income; and many years must elapse before the children can be put to work.' It will be observed that Mr. Godwin does not include in his definition of *providence* any thought on the part of the poor concerning the number of children they shall bring into their own unfortunate position. In fact, this view of the question is studiously avoided by almost all writers even of the greatest eminence, except by those who, like Mill, Darwin, and a few others, adhere to the truths pointed out by the great discoverer of the law of population, Malthus.

"Great towns, then, cause consumption and deterioration of health, by reason of the unwholesome occupations in which they abound. Nor are these unwholesome occupations well remunerated. The fact of their being unwholesome keeps all persons who can afford a choice out of their ranks; but the destitute offspring of a Hampshire or Wiltshire country labourer, with a large family, has no choice. He is penniless, and without even

the rudiments of education, and is therefore glad to accept any employment, however deadly, from the grinders' trade of Sheffield to that of the slopworker of Whitechapel, and thus helps to recruit the large ranks of the victims to this fell disease.

"It may be seen that the amount of pulmonary consumption in this country is in great measure directly attributable to the low wages of the unskilled rural labourer; and the unhealthy towns merely allow a greater number of persons to live a few years in a frail state, without really improving the condition of the working classes. Consumption is one of the positive checks, and the saddest of all, to population; and has in great measure taken the place of those rapid epidemics which used suddenly, as they still do in India, to decimate a half-starved, because redundant, population. Mr. Darwin, in his 'Origin of Species,' as well as Mr. Huxley and Sir C. Lyell, have laid great stress upon the enormous power of reproduction possessed by all animals and vegetables, when placed in positions where their food can be supplied *ad libitum*. The race to which we belong is no exception to this rule, and we have at this day notable examples to illustrate its truth, in the enormous rapidity of reproduction of the inhabitants of the United States and our own Australias. We have there seen the fecundity of that branch of the race to which we belong so great as to enable the early colonists of Massachusetts, &c., to double their numbers independently of immigration, and by the mere powers of a fecundity of which the human race is but one example, in periods in many cases less than twenty-five years. Thus, in the United States before the year 1783, according to J. Garnier, in his work 'On Population,' war and different circumstances were an obstacle to immigration, and carried off from that country more than Europe furnished it with. The French Revolution was an occasion of migration to America; but this movement was soon interrupted by the war in 1793; and from this time until the Peace of 1815 Europe furnished very few emigrants to the States.

"In 1782 the population of the States was 2,389,000
1790 „ „ 3,929,000
1800 „ „ 5,305,000

or more than double in *eighteen years*. This great fecundity of the human race has long been known to be the cause of the low wages which it has been my endeavour to prove to be the main cause of consumption at this time in this country. 'Nature,' says the great discoverer of the *law of increase*, Mr. Malthus, in his "Essay on Population," 'cannot be defeated in her purposes. The necessary mortality must come in some form or other; and the extirpation of one disease will only be the signal for the birth of another, perhaps more fatal. We cannot lower the waters of misery by pressing them down in several places, which must necessarily

make them rise somewhere else. . . . I believe,' he adds, 'that it is the intention of the Creator that the earth should be replenished: but certainly with a happy population, not an unhealthy, vicious, and miserable one.' Unfortunately, the poor are but too often misled by the educated, as to their duty with regard to posterity, as is forcibly remarked by the most eminent thinker of his time, Mr. John Stuart Mill, in his 'Principles of Political Economy:' 'While a man who is intemperate in drink is discountenanced and despised by all who profess to be moral people, it is one of the chief grounds made use of in appeals to the benevolent, that the applicant has a large family and is unable to maintain them. Little advance can be expected in morality until the producing large families is regarded with the same feelings as drunkenness or any other physical excess; but whilst the aristocracy and clergy are foremost to set the example of incontinence, what can we expect from the poor?'

"I have not been able to ascertain the number of deaths from consumption in France. If we measure these, however, by the rate of mortality (by Dr. Christison's method) prevailing in that country, there has been a great diminution of late years of the ravages of the disease in France. Before the Revolution, the average of life, *vie moyenne*, according to the 'Annuaire du Bureau des Longitudes,' was 29 at birth, and it is now 39. It is interesting to look at the concomitant circumstances. M. Joseph Garnier tells us, that from 1806 to 1810 there was 1 birth to 30 inhabitants in France; from 1826 to 1830 there was 1 birth to 33; from 1841 to 1845 there was 1 birth to 35; and from 1846 to 1850 there was 1 birth to 37 inhabitants. This explains the increased comforts of the French population. The capital of the country is increasing, and the numbers remain nearly stationary, hence the average dividend is increasing.

"I have entered thus at length into the question of the dependence of wages on numbers and capital, in order to give the theory of the causation of the great mass of the cases of the consumption occurring in this and similar industrial countries. The theory is, I believe, so well made out, that we may look forward very hopefully to an immense diminution in future of the mortality from such diseases as are caused by overcrowding and unhealthy town-occupations, until, at length, society may solve the problem how to keep the producer in the highest health and at the same time to increase the productive powers of labour.

"A remarkable confirmation of the truth of the account I have given of the causation of consumption has recently been furnished (unconsciously, I am informed by the author), from the answers obtained by Dr. Edward Smith to his numerous questions put to 1,000 hospital patients affected with phthisis pulmonalis. Thus, he found the notable fact, that the average number of children which the parents of these patients

had produced was 7·5. In some of the families there were as many as twenty-three children. It is easy to conceive to what privations in early life many of these unfortunate patients must have been exposed, from the fecundity of their parents."

The truth is, that the prevention of pulmonary consumption, in so far as this disease depends on poverty, the results of over-population, does not lie immediately in the hands of physicians. Medical men can, at the best, only indicate, as Dr. Drysdale has done, this cause of disease: before the cause can be removed the masses must be awakened to the fact that the reckless imprudence, the criminal selfishness, which prompts them, regardless of consequences, to propagate a progeny they cannot feed, shows a want of morality as great as a life of debauchery and vice.

There lies within the domain of medicine, more strictly speaking, a cause of pulmonary phthisis, which we are surprised that Dr. Drysdale has not mentioned. The name of Dr. Drysdale is already known to the readers of this Journal as one who has spoken boldly and strongly against the abuse of mercury, we are surprised therefore, that he does not allude to this, or that he does not dwell more upon it in his pages. We would hope that he may, on another occasion, deal with this important part of the subject, for it is our firm conviction that, of all the causes of consumption which can be regarded as preventable by medical men themselves, this is the most important. We recommend Dr. Drysdale's pamphlet to our readers as one which, without being diffuse, contains much important information.

Our readers will not require from us a lengthened notice of Dr. MacCormac's work, the second on our list; it has reached a second edition, and been thought worthy of translation into two continental languages. Moreover, its talented author, having convinced himself of the immense saving of life which would follow the practical adoption of his views, has considered it his duty, not only at much trouble, but, we must add, sometimes in the face of very uncourteous criticism, to draw the attention of scientific societies to his law:—

"*Wherever the air, habitually respired, has been respired in whole or in part before, there tubercular deposits are found; and wherever the air, habitually respired, has not been respired in whole or in part before, there tubercular deposits are impossible, and consumption and scrofula are unknown.*"

Tubercle he believes to consist of the hydrocarbonaceous waste of the tissues, deposited in the lungs or other structures instead of being burned off as it would be by an adequate supply of oxygen. Several works on the treatment of consumption have lately issued from the press. If these be compared with those which appeared some years ago we feel convinced that nothing will strike the reader more forcibly than the way in which the writers insist on the pre-eminent importance of free, fresh, open air. In the streets of every large city we constantly meet with phthisical sufferers sent out by their medical advisers, who, some years ago, would have been warned not to trust themselves beyond the equable temperature of their room; and in the work which is recognized as the exponent of advanced medical opinion we find it stated that:—"The broadest fact established regarding the exciting cause of the tuberculous deposit is, that the domesticated animal is more liable to tubercular disease than the same animal in a wild state—no doubt due to deficient ventilation and the abeyance of normal exercise of the pulmonary function."—Aitken, second edition, Vol. II., p. 412.

These and many other facts which could be brought forward prove the soundness of the practice for the enthusiastic advocacy of which we believe both the profession and the public are greatly indebted to Dr. MacCormac. We would not, however, be understood to commit ourselves to the admission, that the breathing of air which has been, in whole or in part, prerespired is the *exclusive* cause of tubercle. There are many difficulties in the way of doing so, to which we cannot here advert; but, admitting Compte's dogma,[a] as quoted by Dr. MacCormac, we believe that his doctrine fails to explain the fact, that of several persons exposed as nearly as possible to the same influences, *quoad* air, one alone may become tuberculous.

We are sorry to find Dr. MacCormac, for whom we entertain the greatest respect, maintaining that the opinion formed by conscientious and painstaking physicians, after years of careful observation in thousands of cases, is as childish and irrational as the "superstition of our ancestors in respect of the king's touch" (page 64), and that "all the cod-fish that swim the mighty ocean, were they converted into oil, could not, would not, relieve or avert, were it but in a single instance, consumptive tubercular decay" (page 148). We do not believe that to deficient supply of food or to hereditary

[a] Car toute la saine logique est reductible à cette seule règle, former tonjours, la plus simple hypothèse compatible avec l'ensemble des renseignements obtenús.

taint, any more than to the breathing of prerespired air, are we entitled to attribute the production of tubercle to the exclusion of other influences. Referring to causation, Mr. J S. Mill observes:— "It is seldom, if ever, between a consequent and a single antecedent, that this invariable sequence subsists." Consumption does not, in our opinion, form an exception to this doctrine, which finds so many illustrations both in morals and physics, and we cannot afford to reject any agent which has shown itself capable of preventing that *concurrence of conditions* which is likely to originate and maintain tubercular deposit. A philanthropic disposition, no less than vast erudition, is reflected from every page of Dr. MacCormac's book, and we commend it to the careful perusal of the profession.

Dr. Pollock's work is a contribution to the natural history of phthisis, for the study of which he has enjoyed very ample opportunities, which he appears to have laboriously improved. Prognosis involves not only a due appreciation of the circumstances under which a disease has arisen and which exert their influence on its progress, but, likewise, the estimate at their true value of the resources of art for its alleviation or cure, and hence the work before us contains much on the etiology and therapeutics of phthisis. Many of the views which Dr. Pollock holds do not differ from those which have guided the opinion of experienced physicians for years past, yet, by formularizing the results of his observations, he has made them available for the guidance of others who have not the impressions left on their mind by long practice to draw upon. In all questions into which comparison enters it is of course necessary to assume a standard, and this he does in "ordinary phthisis," the average duration of which he takes at a little more than fifteen months; but as his figures "represent merely the length of time for which the case had lasted from the earliest symptoms up to the latest period while under observation," the actual duration would be considerably longer. He, wisely we think, limits the term "premonitory stage" to that condition of emaciation, *malaise*, and fever, which, in his opinion, precedes the local mischief. On this point his views are somewhat at variance with those brought forward by his former colleague, Dr. E. Smith, in his work on the Early and Remediable Stage of Consumption. As is generally admitted, he shows that these symptoms possess a serious significance when they arise during convalescence from an acute disease, after an exhausting labour or prolonged lactation, or in a member of a phthisical family, but we doubt whether "the existence of the rheumatic diathesis, in

the individual or his parents" would generally be considered as lending probability to the diagnosis of phthisis in a doubtful case. In a special chapter he again insists on this connexion, and states that gout and tuberculosis are similarly allied, a conclusion which is certainly at variance with the opinion of Ancell, Walshe, and others.

Professor Laycock, who has done much to unravel diathetic states, particularly describes the peculiarities of the arthritic tubercular cachexia, and we can quite believe that when the rheumatic element *is* present in a phthisical patient it may give a peculiar character to the inflammation surrounding the deposit, and may render the disease less rapid, though, at the same time, less under the influence of remedies, and specially intolerant of cod-liver oil; but that the existence of the rheumatic diathesis in the individual or in his parents should invest with increased suspicion the symptoms of emaciation and pyrexia we cannot admit without further proof than is afforded by Dr. Pollock's statistics. He found rheumatism had preceded in 49, and succeeded to phthisis in 18, cases in a total of 4,530; so that if we take both together the connexion did not exist in more than one and a-half per cent. of his cases. The figures given in Dr. Cotton's lectures are even more adverse to the idea, as he had only six rheumatic cases in 1,000 phthisical.

The first departure from the course of ordinary consumption which the author notices is "acute phthisis," characterized by undeviating continuity of the morbid actions, and met with under two forms:—

1.—The *Acute Inflammatory*, in which the local disease is a combination of inflammatory products and of tubercle, as an example of which he quotes Dr. Law's case, recorded in the Proceedings of the Dublin Pathological Society, and published in the number of this Journal for May, 1864. Such cases are characterized by all or most of the following symptoms:—Rapid waste; continuous febrile disturbance with exacerbations; early gastric and intestinal derangement; marked hereditary taint, and in females, non-appearance or suppression of the catamenia—the physical signs are those of diffused deposit and of inflammation; they rapidly change, and those of all stages are found simultaneously in one or both lungs. The progress of such cases is entirely uncontrolled by remedies. The rapid softening of a limited deposit in one apex, or the sudden breaking up of an old one, may produce constitutional symptoms almost as alarming; but the history of the case and the limitation of the

physical signs will, with care, enable us to avoid so serious an error as mistaking such a case for one of acute inflammatory phthisis.

2.—A passive exudation of crude tubercle invading the whole of both lungs, with slight febrile disturbance. Of this he gives an interesting case. A delicate looking lad, with strong family tendency to phthisis, while spending the winter at Rome, began rather suddenly to lose flesh and strength without loss of appetite and without cough, expectoration, or hemoptysis; dulness on percussion, becoming gradually more marked, extended over the whole chest; the respiration at first deficient, was dry and tubular throughout, the voice bronchial. There was remarkable impairment of the mobility of the chest on both sides. Dyspnea on exertion was the only distressing symptom, and there were no secondary affections. Death took place within three months; and the entire extent of both lungs was found solidified by grey and yellow tubercles, without inflammatory products or softening; of such cases Dr. Pollock says:—"The prognosis is to be derived from its diagnostic features, which are peculiar and rare. The universality and rapidity of the deposit in both lungs, and the progressive waste of the system, the hereditary taint, and the absence of chest symptoms, and of acute fever, are the leading characters. I have never known such cases either to pause or recover."

Returning to ordinary phthisis, Dr. Pollock maintains that the disease may be arrested in its first stage, and the deposit absorbed, and gives the following summary of the conditions favourable to such a result.

"1. The nature of the tubercle deposited. If there be evidence from physical signs that we have a case of grey, scattered tubercles, few in number, as evident by alteration in the tone and duration of the respiratory sound and by slight dulness, we are to regard the case as favourable.

"2. The absence of signs of irritation in the lung, crepitant râles, much dulness, the various rhonchi, &c.

"3. Phthisis not inherited; the disease accidental;[a] occurring in an individual of healthy antecedents; well-made chest; no previous syphilis nor courses of mercury.

"4. The constitutional disorder soon subsides. This is important; the disease may commence with very acute symptoms, which is not unfavourable, provided they decline early.

"5. One lung only affected; the right the most favourable.

[a] Stokes: "All the cases which recovered, and they were not a few, were cases of accidental phthisis."—MS. Notes of Lectures.

"6. To these conditions must be added the possibility of the patient being placed in the best hygienic conditions, giving up all unwholesome occupation, and leading an open-air life, with liberal diet, &c.

"*A concurrence of all of the above conditions is necessary to a favourable prognosis.*"

The next most favourable result, if the deposit fail to be absorbed, is that it should be tolerated, and Dr. Pollock, in the eleventh chapter of his work, describes cases, seldom met with in persons under twenty or above forty-five years of age, in which a limited tubercular deposit receives no increase, and undergoes no change for an indefinite period; the most hopeful cases of this group are usually of the lymphatic temperament, without hereditary taint, with a fairly good digestion, and without the indications of local or general irritation, but there are other cases which, though claiming admission into the same group, hold their place there much less securely; those, namely, who are at intervals reminded by sub-febrile attacks and symptoms of pulmonary irritation that the disease, though generally quiescent, has not left its ground. In the twelfth chapter, Dr. Pollock gives us the result of his experience of cases in which cretaceous expectoration occurred, and which were generally slow in their progress. We may allude here to the treatment which, in one of the concluding chapters of his work, he recommends with reference to the special indications in the first stage:—

"Local depletion," he says, "cannot be too strongly urged as the necessary, if not the sole, mode of relief in the early days of a declared deposit in the lung, with irritative fever.

"The common case of a florid hemoptysis, with high pulse, a daily febrile access and sweating, where dulness and feeble respiration, with prolonged expiration, are ascertained to exist at one apex, ought to be actively treated by local depletion, followed by counter-irritation. A few leeches followed by the cupping-glass over the seat of dulness, saline medicines, perfect repose, and a non-stimulant diet, offer the best chances of ensuring quiescence of the deposit which has already taken place. At this period it is vain to put in practice the system of feeding the blood which we have advocated in the premonitory stage. Our question is whether this local irritant shall proceed to distructive changes necessarily involving the lung, or be tranquillised into the condition of a foreign body tolerated in a vital structure. On this depend our hopes of chronicity, and the chances of absorption. Absorptive processes do not occur so long as local irritation remains. The changes for which we look in the most hopeful condition of deposited tubercle only take place when pulmonary irritation or

inflammation has ceased. Cod-liver oil and iron, with tonics, cannot control or immediately alter the deposit, nor prevent its remaining as a mechanical irritant. Neither can the stimulant and supporting treatment so commonly applied to every affection involving exhaustion of vital powers, be here available, or indeed be regarded as other than mischievous.

"When the local irritation has subsided, and the febrile symptoms lessen, the pulse sinking to a natural standard, and the digestive system becomes improved, as evidenced by a cleaning of the tongue, oil may be commenced with the best effects. Its use is perhaps best preceded by the lighter tonics, to which an alkali may with advantage be added."

On the importance of availing ourselves of the first remission in the progress of the disease, Dr. Pollock makes most judicious observations.

"After such local irritation has subsided, we generally find that the system is in a condition to take up nutritive matter very actively. . . . It is remarkable with what ease the blood may be fed, and all the natural indications are for abundant nourishment and such assistance as we can afford by oil, iron, and tonics. We cannot speak too highly of the adjuvants of open air, full exercise, mental employment short of fatigue, the cold sponge bath, and an abundance of the more nutritive articles of food. By following this method, and by the use of prolonged counter-irritation, we have seen cases of the most threatening appearance become chronic, and in some instances the physical evidence of local disease disappear."

The stage of softening, as well as the first stage of phthisis, may be very prolonged, and according to Dr. Pollock, we may hope that such will be the case when the surrounding congestion and irritation subside, the general health improves, the pauses in the progress of the disease become lengthened, and the sounds in the chest become dryer; he draws special attention to "Chronic Diffused Tubercle;" he gives it as the result of numerous observations, "that tubercle when diffused or spread throughout a large surface of one or both lungs undergoes its distructive changes slowly, and in certain instances dries up;" and that this condition is, within certain limits, favourable for chronicity in the first and second stages of the disease, but unfavourable in the stage of excavation; in the thirteenth chapter of the work will be found some interesting observations on the symptoms and physical signs of the form of phthisis, as well as on the circumstances which would in any individual case justify a favourable prognosis.

In the twenty-seventh chapter are found his directions for the

treatment of the second stage; they are those usually given in works on this subject. Contrary to the experience of Walshe and other observers, Dr. Pollock has found quinine sometimes useful against the hectic chill; and from iodide of potassium he has seen excellent results in certain cases of spasmodic dyspnea; when opiates are necessary, but derange digestion. "The best antidote is lemon juice or a small cup of strong coffee taken on waking." Dr. Pollock then proceeds to show that the stage which is the last in the series of pathological changes, may likewise be very greatly prolonged; while the cavity is forming, the position of the patient is sometimes critical; but once this has actually taken place, local irritation and constitutional suffering may both subside; the circumstances which of course mainly influence the result are the intensity of the constitutional predisposition, the external circumstances of the patient, the degree to which the local mischief has extended and the amount of conservative energy in the organs which maintain innervation and digestion; for the details, we must refer to the work itself, where a very elaborate table of the prognostics is given.

Several pages are devoted to digital clubbing, which Dr. Pollock has found associated almost exclusively with that form of phthisis in which, though more than one cavity has formed, toleration of the disease may be expected; on the treatment specially required in this, the third stage, some remarks will also be found towards the close of the volume.

A chapter is devoted to phthisis accompanied by external struma. He has found some peculiarities in its localisation and progess, which appears to have been somewhat slower than ordinary phthisis. In persons above forty-five years of age consumption is, on the whole, slow; but Dr. Pollock adds the weight of his experience to the opinion of those who consider cod-liver oil is greatly less beneficial in the aged, and travel is not, he thinks, to be recommended to those who have passed sixty. As is now generally held, he views any attempt to heal fistula in ano in the consumptive, as most injudicious; and believes that whatever temporary power pregnancy may have in rendering the disease quiescent, labour and suckling generally lend to it a fatal rapidity.

The chapter on Pneumothorax contains some interesting observations; as do those on Hemoptysis, Hereditary Transmission, and the Influence of Occupation on the Phthisical; though on these subjects Dr. Pollock's views are simply confirmatory of those usually held.

Under the head of Preventive Treatment of Phthisis in Infancy

and Childhood, Dr. Pollock gives much judicious advice as to the best means of warding off the disease from those who inherit a tendency to it. He dwells on the necessity for good food, the danger of lowering treatment when they are attacked by illness, the importance of air, light, bathing, and suitable clothing, the imprudence of over-taxing their powers, either mental or bodily, the evil results arising from the abuse of stimulants and tobacco, and the importance of selecting a profession favourable to health. Those practitioners who take up Dr. Pollock's book to gain, from a hasty perusal of this chapter, some suggestions for their assistance in such cases, would, we think, be led into an erroneous practice in one respect. At page 355 he recommends that the children of those who can afford to go abroad, should spend their Winters in one of the health-resorts of Italy or France, which permit of horse exercise at that period of the year. From some remarks elsewhere in the work, we are disposed to think the author's views on this subject are not very different to our own; but as the paragraph stands it would lead others to give advice from the adoption of which evil instead of good would, we believe, generally follow. The benefit to be derived from change of air and scene may occasionally render a Winter tour in Southern Europe desirable, but as a rule, we are convinced that at this stage, when we have to deal not with a local disease but with a condition of low vital action, it is a positive and great injury to a youth to lose the more bracing effects of our northern Winter, the vicissitudes of which may, with ordinary caution, prove of real service to him. Those youths who, *fruges consumere nati*, can live where they like, are more likely to find vigour and strength, while they seek only amusement, on the moors or in the hunting field at home, than in the *conscious* pursuit of health over the weary waste of the Campagna, or killing time, as best they can, among the acknowledged valetudinarians of Pau or Mentone.

The chapter on the general, or as it is termed the direct treatment of phthisis, may be read with advantage by those who are still disposed to trust to drugs, to the neglect of open air exercise, gymnastics, and healthy sports.

On the subject of climate, to which is devoted the last chapter of Dr. Pollock's work, we shall offer a few remarks in connexion with the book which is next on our list.

The introductory chapters, which we have purposely postponed noticing, contain general considerations on chronic organic disease,

and on the resistance of the system; on the mode in which the *vis vitæ conservandæ* shows itself by compensatory arrangements and efforts to limit disease, a subject of great interest and practical importance, but requiring, as every attempt at generalization does, a great accumulation of facts collected in different departments of observation, and the rare power of keeping these clearly before the mind; and we think Dr. Pollock has rather argued from the particular, with which his inquiries have made him most familiar, to the universal. There is one pet theory he has got, which, after the reader thinks he has discarded it for the ordinary view, crops up again towards the close of the book, and we feel constrained to notice it because we think it not only fanciful but liable to mislead At page 20 we find the following sentence:—"In tracing the analogy between the natural processes in a pneumonia and in a deposit of tubercle, as indicative in both of a conservative energy on the part of the system, it must be remarked that in both the febrile disturbance seems to have a common object, viz., an abatement of the requirements of the blood for air, and the necessary reduction of the tissues;" and at page 23 the author thus expresses himself:—"Nor are the systemic changes less characteristic of resistance to the progress of chronic morbid changes than the local. The reduction of the solids of the body, the emaciation, muscular waste, and absorption of fat, are necessary (as already stated) in order to establish a balance between the system as a whole and the organs which are to supply it." The true nature of pneumonia is still *sub judice*. Competent pathologists are not agreed as to whether the pyrexia has its analogue in the fever which follows a local injury or the pulmonary lesion, its counterpart in the articular inflammation, in the production of which the blood-poison of gout expends itself; but we do not see that Dr. Pollock's idea of the physiological import of emaciation would fit into either theory. In phthisis emaciation often precedes, by a considerable interval, the local mischief; and even if it did not, we cannot conceive that nature means to lessen the labour of the oxygenating organs by loading the blood with the hydro-carbonaceous products of waste.

In a monograph on pulmonary tuberculosis we were surprised to find no mention made of the peculiar form of dyspepsia which so frequently accompanies the cachexy, nor of the alterations in the temperature of the body, which have been shown to mark the commencement and increase of the deposit.

The question of change of climate for a consumptive invalid is

one of the most anxious which a medical adviser has to determine—a resource which undoubtedly does accomplish what no other means will, should not lightly be neglected, but a step which in unsuitable cases hastens death, and deprives the sufferer, in the last stage of the complaint, of the comforts of home, must not be recommended without serious consideration. Formerly it was a hope to which the wealthy alone could cling, but now in our Australian colonies the poor man may seek to better alike his health and his prospects. We are convinced Mr. Brown's little work will prove useful. It reminds us of the fact which every one knew, but which many did not sufficiently consider, that it is a matter of no small importance, as far as regards health, for the invalid to make a careful selection among the various climates of Australia.

"Tasmania," he says, "may be considered *the* climate *par excellence*, not only of the Antipodes, but, I believe, of the universe, for that class of the English consumptive invalids who require a climate more equable than our own, but not of much greater temperature, and with an ozonized, clear, and bracing atmosphere." "The neighbourhood of Hobart Town, and the mountainous districts, is to be preferred to that north and north-west of the island, where, as in Launceston, the land is lower and in some places marshy. New South Wales is adapted to those cases which require a temperature warmer than the one in which the disease was manifested; and, as a Winter or Spring climate offers, in combination with residence in Tasmania (during the Summer months), many advantages—Sydney, however, is the least agreeable portion of this colony—Goulburn, Bathurst, the Illawarra, and Hunter districts, each present to the traveller various climates, scenery, and occupations."

Mr. Brown advises invalids to go to Melbourne in a comfortable sailing ship, and hence by the inter-colonial steamers. Having had some experience in such matters ourselves, we can say that the invalid bound for Australia will find in his work many valuable hints, by attention to which the comfort and good effects of the voyage will be much increased. His observations on Australian prospects may be read with advantage by others. The book is intended for the laity, and does not, therefore, contain much information on the conditions which should lead us to advise change of climate. On this subject we consider Dr. Pollock's remarks very just. He shows that the cases of phthisis benefited by change go away when the irritive fever is absent, the local disease not

advancing, and the digestion tolerably healthy; that the Spring, and not the Winter, is the dangerous season for England, and that a pure dry air of moderate temperature is the one to be sought for. Both he and Mr. Brown strongly urge a long sea voyage as of inestimable benefit in many cases, an opinion in which we fully concur.

"Emphysema, its Pathology—and Bronchitis, its Treatment," or some similar title, would have conveyed to his professional brethren a more correct idea of the scope of Dr. Dobell's work than the one which his modesty, no doubt, has led him to select. WINTER COUGH, however, may lead a larger number, if not a more discriminating class, to interest themselves in the book and its author. Rejecting the theory of disease of the walls of the air cells, and also that of compensatory distention during inspiration, as explanations of the mechanism of emphysema, Dr. Dobell follows Dr. Jenner and others in believing that the injurious pressure is exercised on the vesicles during expiration; and having shown that this pressure comes to be exercised owing to obstruction to the outward current, he draws attention to some hitherto unnoticed conditions, which are liable to give rise to this obstruction—nasal and post-nasal catarrh, catarrhal thickening of the mucous lining of the larynx, trachea, and large bronchi. As everyone knows, however, repeated attacks of bronchitis, more frequently than all other causes, lay the foundation of emphysema; and under the head of treatment, Dr. Dobell insists on the necessity of curing bronchitis completely, and taking precautions against its return. The book has the merit of being clearly written; and those who may not agree with the author's conclusions will have no trouble in following his reasoning, and may get useful suggestions from his chapters on treatment.

Laryngoskopiske Iagttagelser hos Dövstumme. Af Dr. L. W. Salomonsen.

Laryngoscopical Observations on the Deaf and Dumb. By Dr. L. W. Salomonsen. Reprinted from the *Bibliothek for Læger*, Jan., 1866. 8vo, pp. 14.

THE author was led, partly by a statement by Dr. Gibb, that in examining two deaf mutes he had found that the chordæ vocales were wanting, partly by the interest he took in the efforts to teach

the deaf and dumb to speak, to undertake the laryngoscopical examination of a number of such individuals.

"As is well known, there are in Copenhagen two institutions for the deaf and dumb. In one—the Royal—so far as I am aware, no other language than that of signs and fingers is employed; no effort is made to teach the pupils vocal language: it is, or at least it was formerly, rather difficult for strangers, even for physicians, to obtain access to the institution. In the other—the private or deaf-mute school—based upon the speaking method, on the contrary, a considerable number (from 70 to 80) of pupils are instructed in and by the vocal method, and the institution is open to every visitor; the manager, Hr. Emil Keller, receives everyone in the most obliging and polite manner, for which I, for my part, return him my sincere thanks. This is not the place to enter into a discussion respecting the advantages and the defects of the vocal method, or to give any opinion as to the efficiency of this school; but this much I may be permitted to say, that although in my mind it does not come up to the ideal of such a school, and cannot, for example, be compared to the deaf-mute institution in Vienna, its whole plan and arrangement are so excellent, and its results so good, that it fully deserves the favourable judgment and the general and steadily-increasing sympathy on which it has hitherto had to congratulate itself. I sought and got permission to make laryngoscopical investigations on the pupils of this school, my object being to see how far the circumstances mentioned by Gibb should prove to be more general, in which case vocal language would for many be, of course, a physical impossibility; or whether, on the other hand, in accordance with the opinion hitherto usually entertained, the vocal organs should appear to be normal, the essential cause of the deaf-mutism being the deafness, while vocal language would not only have the natural physical condition for its possibility, but would also present itself as that which ought to be aimed at. I shall not delay to describe all the difficulties, on the patients' side, with which I had to contend in endeavouring to carry out the laryngoscopical examination; anyone can imagine how much the patients' want of hearing, the absence of control over the tongue, their fright, their uncertainty as to the directions given, &c., &c., must interfere with our efforts, and how much time must often be consumed in preliminary exercises. I shall only take leave to mention the, so far as I can judge, very satisfactory results I have obtained."—Pp. 123-125.

All the examinations in general exhibited one and the same perfect or more or less normal condition of the vocal organs. Dr. Salomonsen lays the results before his readers in a table which

occupies upwards of six pages of his very interesting essay. Upon these results he makes the following remarks:—

"This table accordingly shows as follows:—Of the 70 or 80 patients in the school 24, or about one-third were examined; they were taken at random, for which reason their ages varied from 9 to 19 years, but the majority were, nevertheless, from the upper classes, and had been a considerable time in the school; the pupils in the lowest class were too small and too nervous to admit of their being examined. Among these 24 there were two who could not be examined with the laryngoscope; in 5 the epiglottis was so strongly directed backwards that it entirely, or at all events essentially, concealed the deeper parts and prevented the accurate investigation of the latter; in 17 all the deeper parts were seen and were found to be all normal; in 14 the chordæ vocales closed even quite to one another in the pronunciation of 'æ,' and only in 3 did a little elliptical slit remain between them. It will be readily granted, that if the subjects examined had been sound individuals, accurate laryngoscopy could have been carried out in more than 17 among 24; but from the remaining 7 we must, in the first place, subtract the 2 in whom the tonsils prevented the examination; and, in the second place, I have no doubt that in the five in whom the direction of the epiglottis made the investigation difficult, I should nevertheless have been able to accomplish it if they had been hearing and speaking persons, or if I had several times practised them in bearing the instrument. The main result, however, stands good; the doubt and reserve which one involuntarily feels in reading Gibb's statement that the chordæ vocales were deficient in the only two deaf-mutes he casually examined, and which Semeleder, for example, has given expression to by placing a note of interrogation after this statement; this doubt must be much strengthened, nay, almost followed by the conviction that Gibb did not know how to overcome the technical difficulties in laryngoscopising these persons, when we see that in the overwhelming majority, not to say in all the deaf-mutes examined by me, not only were all the constituent parts of the vocal organ present and completely normal, but also that the chordæ vocales in uttering a sound perfectly executed the normal movements. In these facts lies not only an incontestable confirmation of the correctness of the original popular assumption that it is essentially the deafness which causes, and carries with it the dumbness; but there is contained in them also, as it appears to me, a weighty argument in favour of the introduction of the vocal method in the instruction of deaf-mutes. . . . But I foresee an objection to the demonstrative force of these investigations—namely, that among the 24 deaf-mutes the deaf-mutism was congenital in only 6; in all the others it was acquired. But, in fact, this objection does not carry much weight; for in the first place we must to these six

add two, in whose cases the age at which the deaf-mutism occurred is unknown. In the next place there are among the others several, at least six, in whom the deaf-mutism is stated to have commenced between two and five years of age, consequently before there could have been any idea of their learning to write; and as to them it is a generally-admitted proposition that the deaf-mutism, with respect to the difficulty of learning to speak, may be placed on a level with congenital deaf-mutism; and lastly, it is certain that similar circumstances are to be met with in every collection of deaf-mutes. . . . Thus Mitchell[a] states that among deaf-mutes the deaf-mutism is not congenital, but is acquired after birth: in Great Britain, in 25 per cent.; in the United States of North America, in 42 per cent.; in Germany, in 52 per cent. Among my 24 individuals it arose, after birth, in 18, or perhaps more correctly, in only 16, that is, in 66 per cent. The difference between this and the proportion in Germany is not greater than might depend upon accidental circumstances, and at all events cannot afford any firmer support to the objection above referred to."—pp. 132-134.

We have shown our sense of the value and importance af Dr. Salomonsen's observations by the length at which we have quoted from them. On one point just alluded to we must, however, take exception to his remarks, namely, where he places deaf-mutes meeting with their affliction at from two to five years of age on a par with congenital deaf-mutes in respect to learning to speak. There can be no doubt that a child who should reach even two years before becoming deaf, would possess a very great advantage in its subsequent education over one born deaf. We were ourselves acquainted with a young gentleman who totally lost his hearing, from fever, at five years of age, but who up to the period of his death, from phthisis, twenty-two years later, could speak so as to be easily understood, while his friends, in communicating with him, were obliged either to use their fingers, or to articulate so distinctly that he could, from the motions of the lips, judge what was said. In this latter mode he was able even, if favourably placed, to follow *some* preachers so perfectly, as to take in the meaning of their sermons.

[a] Annales d'hygiène publique et de médecine legale. July, 1865, p. 77.

PART III.

MEDICAL MISCELLANY.

Reports, Retrospects, and Scientific Intelligence.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.[a]

Dr. M'Dowel, President.

Scirrhous Tumour in the Mammary Region.—Mr. Tufnell said he brought forward the specimen now before the society, as a case not of cancer *of* the breast, but as a cancerous tumour *in* the breast, which he thought would be interesting to surgeons, with regard to the question of removing the breast itself in cases of malignant tumours. The individual from whom he had taken the morbid specimen was a woman fifty years of age, and in the rudest health, being apparently not more than thirty years old. About August last she fell against a hard body, and gave the breast a severe blow. Early in November she first noticed, below the left clavicle, a tumour about the size of a pea. This gradually increased until it attained its present size, viz., that of a small apple. It was a densely scirrhoid body. The mamma itself was perfect, the tumour separated from it entirely, growing from above downwards, and from without inwards, until it at last became adherent to the cartilage of one of the ribs; its base was polished as if lined with a serous membrane. The tumour was so hard that it had quite the feeling of an unripe pear. Under the microscope it gave the ordinary characters of scirrhus. Mr. Tufnell presented it to show that it was a tumour in the breast, but not a cancer of the mammary gland, which was perfectly healthy. It was a case which proves that although a cancer might be in the vicinity of the breast, it was not always necessary to remove the mamma itself, as some surgeons maintained, and carried out in practice.—*December* 9, 1865.

Epulis.—Dr. Fleming exhibited a cast and drawing illustrative of the appearances present in a case of epulis in a boy aged between five and six years. The disease occupied the left side of the lower jaw, in the interval between the first and second molar teeth both of which were displaced and imbedded in the morbid growth

[a] These reports are furnished by Dr. W. Smith, Secretary to the Society.

The history of the commencement of the disease was unsatisfactory, as well on account of the age of the child as of the fact that the mother had only accidently discovered it about a month back, when, perceiving a fulness in the cheek of the child, and examining the mouth, a tumour attracted her attention. The child complained of no uneasiness from the affection; he eat and drank as the other children, neither was articulation remarkably affected. She never observed any bleeding or any discharge of any kind from the mouth, and the appearance and the condition of the child indicated ordinary good health. The drawing, by Mr. Connolly, and the cast, for which Dr. Fleming was indebted to Mr. Grimshaw, gave a very accurate representation of the locality and of the appearance of the morbid growth. It occupied the space between the first deciduous or temporary molar tooth and the first permanent molar. The second deciduous molar was not visible. The physical characters of the mass were much as those of the ordinary gum structure—the colour and the consistence being identical; in size and outline it much resembled a small tomato fruit, the under part of which would be as if sulcated to embrace the corresponding alveolar portion of the jaw, whilst its lateral portion overlapped the inner and outer projections; the different surfaces of the tumour were nodulated and irregular, the outer bulging less than the inner, which encroached so much on the floor of the mouth as to displace the tongue, and push it up to the right side, and towards the hard palate. The covering of the tumour was unbroken over the mucous membrane; it was devoid of pain on pressure, and free from any trace of ulceration. There had never been any hemorrhage from it. The teeth contiguous were slightly displaced and loosened in the sockets, but otherwise they were sound.

When the child was admitted into hospital, he had an eczematous eruption about the face, which delayed any operative interference for some time. During this there was not any appreciable increase in the tumour. Its removal was decided upon, commencing with that of the contiguous teeth. A knife, shaped somewhat like a farrier's draw or hoof-paring knife, was used for the purpose with great advantage. And this was done on a level with the inside of the adjoining gum. The alveolar portion between the teeth removed, was freely excised with a strong-backed scalpel. Sharp hemorrhage followed, which was quickly controlled by ice and ordinary astringent applications. In the process of healing, some portions of the alveolar sockets exfoliated; and when the boy left hospital there was every indication of a satisfactory cure.

A knowledge of the pathology of this disease is indispensable to its radical extirpation; and when we reflect on its anatomy, involving as it does the gum structure, the periosteum, and the peridental membrane, much care is required. The peculiarities in this case are, the apparently rapid growth of the disease, its engaging both lateral aspects of the body of the jaw, and perhaps its special locality. None of the symptoms usually attendant on malignant disease were present

Through the kindness of Dr. Barton, Dr. Fleming was enabled to exhibit a drawing of a case which occurred under his care, in a little girl aged three years, where an epuloid growth existed, which was successfully removed, but where the external features of the disease had an apparently different character. The colour of the new growth was very peculiar as contrasted with that in the case now exhibited to the society, and the displacement of the teeth was greater. Here the teeth were removed, and also the adjoining portions of the alveoli. It appeared to Dr. Fleming to belong to that form of the disease which is termed myeloid.

The accompanying woodcut, by Mr. Oldham, represents accurately the morbid growth.—*December* 9, 1865.

Malignant Tumour.—Mr. Croly exhibited a tumour which he had removed from the neck of a woman in the City of Dublin Hospital on the previous Thursday. The patient, forty years of age, was of sallow com-

plexion, but had in general enjoyed good health. The disease commenced to grow between two and three months previously. It soon became as large as a tennis ball, was never very painful, but occasionally she felt a sting down her arm. She could not lie comfortably on her right side as symptoms of suffocation were produced by that posture.

The tumour quickly increased in size, and, on admission into Hospital, was as large as the section of a small orange, convex and even on the surface, situated beneath the ramus of the right side of the lower maxilla, extending upwards near the mastoid process, downwards beneath the angle of the jaw, backwards close to the spinous processes of the cervical vertebræ, and downwards and forwards to the submaxillary region; it had a fluctuating feel and was partly movable. On consultation, it was decided that the growth should be removed, which was accordingly done by Mr. Croly. The necessary dissection was difficult, and exposed the sheath of the carotid artery; the diseased mass was removed with the fingers, and a portion which extended deeply backwards to the transverse processes of the vertebræ was ligatured. The microscopic examination of the morbid structure showed a soft fatty material like marrow, and numerous nucleated cells. There could be no doubt of its being of the nature of medullary cancer.—*December* 16, 1865.

Mitral Valve Disease; Pericarditis.—Dr. Law said the morbid specimens which he now submitted to the society, was a complication of pericarditis, mitral valve disease admitting of free regurgitation and extensive hypertrophy. The subject of the case was a boy twelve years old. About a year previously he had consulted him for headache and palpitation of the heart. The affection of the heart then appeared to be enlarged mitral orifice, admitting of free regurgitation. The grounds of his diagnosis were these: the abnormal sound heard in the situation of the apex of the heart, externally on the left side, and posteriorly between scapulæ. There was also extensive dulness on percussion in the cardiac region, especially in the transverse direction. These symptoms he thought conclusive as to the nature of the lesion; the abnormal sound, the increased dulness on percussion, and the strong action of the heart. The boy complained of headache which Dr. Law had no doubt was a mere rheumatic headache; for he had originally had rheumatism, and upon that followed the affection of the heart. When he came under his care last, on the 12th of May, what he then chiefly complained of was distress in his breathing and general discomfort, not actual pain. His pulse was about 132, there was not much heat of skin, but there were the same cardiac signs that existed before, namely, an abnormal sound in the situation of the apex of the heart, heard extensively over the side, and posteriorly. At the same time he had pain in his ankles, but without swelling or redness. These pains yielded to stuping. He complained still of

considerable distress about the cardiac region. First of all Dr. Law only heard strong, quick, and rather tumultuous action of the heart. On the third day of his admission he discovered a friction sound in the region of the heart. This sound became more distinct and more extended day after day; and each day, for a period of ten, they had an opportunity of hearing, all through the cardiac region, different modifications of the friction sound, sometimes coarser, sometimes finer; but for ten days the friction sound was never absent, varying in different situations. There was fulness in the cardiac region, and a distinct vermicular motion perceptible. After ten days the friction sound entirely disappeared. While present it entirely masked anteriorly the abnormal sound that indicated the valvular lesion; but this was always heard posteriorly. When the friction sound disappeared, there was a strong action of the heart, from which Dr. Law considered that adhesion had taken place, and he regarded this increased action of the organ as an effort to relieve itself of this impediment to its action. On the second day after the cessation of the friction sound, the symptoms underwent a very great aggravation. His dyspnea was greater, his face became puffed, his legs swelled, and general dropsy set in. His appetite failed, and he went on from bad to worse, and so expired. His illness altogether occupied about three weeks. He was disposed to consider this aggravation of the symptoms to depend on the impediment to the heart's action in consequence of the adhesion which he expected to find.

The *post mortem* examination exactly confirmed the diagnosis in every particular. The heart when taken out weighed twenty-eight and a half ounces; the average weight of the adult heart being only nine. The cavity of the pericardium was completely obliterated, in parts its sides admitted of being separated, and the whole organ was found coated with lymph varying in depth and in density, and Dr. Law thought he could directly connect the different appearances of the lymph with the coarser or finer *frottement* existing during life; towards the apex, the lymph being finer, showed there was more motion and therefore the more friction; and towards the base less fine from there being less motion and consequently less friction of the surfaces against each other. Immediately under the lymph, and closely applied to the heart, there was a dense fibrous substratum indicating successive attacks of pericarditis. It was exactly what Rokitanski remarked when he described the textural development that fibrin, the result of inflammation, undergoes, that it becomes the seat of chronic inflammation which consists of a linked succession, stage for stage, of inflammations, more or less acute; amongst the most remarkable of which were those occurring in the vascularized product of an antecedent inflammatory process, more particularly in the pseudo-membranous products of inflammation on serous membranes. Dr. Law remarked that he had often seen exhibited at their meetings, specimens of

what were called hemorrhagic pericarditis, but which from the depth of the false membrane, the seat of the hemorrhage, and its density, must have had an origin long anterior to the present inflammation. He remembered that some time since Dr. Byrne exhibited a case of hemorrhagic pericarditis, in which there was recent lymph with some blood and a substratum of fat. He was satisfied at the time that in this case there had been a succession of inflammations.

As to the condition of the mitral orifice, it was very much enlarged, so as easily to admit of three fingers being passed into it; nor were they left to conjecture as to the course of the blood when it passed in greatest quantity; for on looking at the aorta they saw how small it was compared with the bulk of the heart. It was a physiological principle that a vessel accomodated itself to the amount of blood that passed through it, and they were justified in believing the blood in this instance flowed freely back. They had an opportunity of seeing the peculiar modification of shape produced by this lesion, differing from that which it undergoes in aortic valve disease; in the latter we had an elongated heart; here we had it hypertrophied in a transverse direction. This, too, confirmed the observation as to the direction the blood took. As in one case hypertrophy was established to sustain the returning column of blood; here on the contrary it was directed against the mass of this fluid, coming from the auricle into the ventricle before its time, so that there was a collision, as it were, between the blood coming from the auricle into the ventricle, and that passing back or regurgitating into the auricle. This explained why the heart was enlarged in the transverse direction in mitral valve disease, and elongated in cases of disease of the aortic valves. This boy was the member of a family consisting of five children, and three of them were at this moment labouring under heart disease; while neither parent was rheumatic.—*December* 16, 1865.

Varicose Aneurism.—Dr. Hayden brought before the society an example of spontaneous varicose aneurism, engaging the aorta and the superior vena cava. The patient was a man thirty-three years of age, a mason by trade, and of intemperate habits. He had enjoyed excellent health up to the last year of his life. During that time he had suffered considerable privations, but still he did not complain of positive illness. About five weeks before Dr. Hayden saw him, it appeared that whilst lifting a heavy stone from off the shoulders of a fellow tradesman he made a false step and strained his back. He complained of acute pain. at the moment, in the dorsal region, and in consequence was unable to work for some days, but then resumed his labour and went on as usual. A fortnight subsequently, while walking in the street, he experienced a sudden sensation of weakness in the lower limbs, and was about to fall, but supported himself: the feeling of faintness passed off in a few minutes, and he was able to proceed.

About this time he called at the Mater Misericordiæ Hospital and was seen by Dr. Hayden: he then seemed to be under the influence of drink. He coughed slightly and was hoarse. He did not think it necessary on that occasion to make an examination of the chest. Three weeks afterwards he again presented himself at the hospital. His condition was then so striking that Dr. Hayden at once admitted him, and after placing him in bed made a careful examination of the chest. The face, neck, and chest were deeply cyanosed, tinged, and œdematous: the face and neck in particular were of a deep purple tint; the conjunctivæ were injected with dark, venous blood; the pupils contracted, but equally so: ears and lips of a deep, leaden hue, and the neck bloated, so that the angles of the inferior maxilla and the supra-clavicular fossæ were obliterated. The cervical veins were distended, and yielded a doughy feel. They were, however, devoid of pulsation and thrill. The chest was covered throughout with groups of tinged and tortuous subcutaneous veins: those of the arms and forearms were also distended, and the finger ends of a dark, livid colour. From the diaphragm downwards the body was of the normal colour and temperature; above this line the surface was remarkably cold, and the man constantly complained of a feeling of chill; but in the lower part of the body he had no such sensation, and the feet were of the normal temperature. There was cough and expectoration. The pulse was 114—small, but regular; the respiration, 24. The tongue was moist; the bowels natural. There was no dysphagia.

On carefully examining the chest he found it unusually resonant; a little less so to the right of the sternum, and from the clavicle down to the nipple. Respiration was remarkably loud over the entire of the left side of the chest, both before and behind, on both sides. It was accompanied by muco-crepitant râle. Over the lower portion of the right side respiration was likewise normal; but above the nipple, on that side, it was feeble, as it was also from the middle of the infra-spinous fossa to the apex of the chest.

There was increased extent of precordial dulness; the heart's action was regular; the impulse strong, but unaccompanied with *fremissement;* the apex pulsated behind the ensiform cartilage. Over the base of the heart was heard a double *bruit de soufflet;* the first or systolic murmur was coarse, loud, and transmitted upwards in the course of the aorta, through its arch and into the carotids; it had its point of maximum intensity at the junction of the third right costal cartilage with the sternum, and was, moreover, diffused over the entire chest anteriorly, but louder on the right side; it was likewise audible in the right infra-spinous fossa.

The second murmur, audible over the lower portion of the sternum, was diastolic, and seemed to replace the second sound of the heart; it was soft and somewhat masked by the former murmur, and strictly

confined to the region of the base. On placing one hand upon his chest in front, and another behind, and desiring him to respire forcibly whilst the hands were in this position, a feeble impulse was felt in the upper portion anteriorly, at the *acme of expiration.*

The condition of the patient remained somewhat the same for several days. On the night of the 20th of October, three days after his admission into hospital, he was slightly incoherent, and on the following morning he (Dr. H.) thought it prudent to extract some blood from the engorged veins. He accordingly had nine leeches applied to the upper part of the chest. The leech-bites bled freely, two of them so much so that it became necessary to apply caustic to arrest the hemorrhage.

On the 22nd of October the man complained of some slight difficulty in swallowing. This difficulty, however, was traceable to the state of tension of the integuments of the upper part of the neck.

On the 23rd the state of engorgment of the veins of the upper part of the body, and the tension of the arms were so great, that he thought it prudent to take some blood from the arm. The tension of the veins of the forearms was such that the application of the ordinary bandage was unnecessary. About eight ounces of blood were taken from him, with the effect of giving him much relief. On the 24th the superficial veins of the left forearm were in a state of phlebitis; they were hard, tender to pressure, and marked by lines of ecchymosis. Six leeches were applied here, followed by warm poultices. The leech-bites bled freely and the man experienced relief. At this time his colleagues and the class were struck forcibly with the appearance of the man. His condition remained little altered till the 27th of October. In the interval an attempt was made to have his photograph taken, but to this the man evinced an unconquerable objection, and the idea had to be abandoned. On the 27th of October he complained of an overpowering sensation of drowsiness, and shortly afterwards died without a struggle.

The chest only was examined. On laying open the thorax it was found that the lungs had completely enfolded the pericardium and were adherent to it. They were engorged with blood and serum, which flowed freely from them on section. They floated in water. The pericardium was greatly thickened and adherent to the heart throughout. The heart was of the normal size and consistence. The right auricle contained a mass of dark, coagulated blood: the superior cava, at its junction with the auricle, was flattened from before backwards, and its two walls attached to one another in the centre. At one side of this adhesion there was a passage into the auricle through which the little finger might be freely introduced, but on the other side of the abnormal septum the opening was greatly contracted, admitting only of the introduction of an ordinary-sized probe. He was at first under the impression that this adhesion was caused by inflammation; but Professor

R. W. Smith, who examined the specimen carefully, had expressed his opinion that it was congenital. The ventricles were normal; the trunk of the pulmonary artery and its valves were healthy, as were likewise the left auricle and ventricle, and the mitral and aortic valves. The aorta was considerably dilated, and its lining membrane dotted over with pale yellow spots.

In the posterior wall of this vessel, and about half an inch above the valves, was a large opening of an ovoid figure. This opening extended across the artery, and measured, transversely, an inch and three-quarters, and vertically one inch. The margins of this aperture were smooth, but still irregular. It led into a large aneurismal sac, large enough to contain a moderate-sized apple. The walls of this sac were in a rugous condition, but composed of the coats of the aorta, the lining membrane of which was traceable into it. The sac contained a few loose masses of fibrin and some dark coagulated blood. This tumour had pressed backwards on the right branch of the pulmonary artery, which it had almost obliterated. It had pressed forwards and outwards upon the vena cava descendens, and occluded that vessel from its entrance into the auricle to a point half an inch below the junction of the vena azygos, and formed a communication with the cava by three openings in that vessel, one of which was the size of a three-penny piece, whilst the others were much smaller. The edges of these openings were thin, jagged, and bordered by red areolæ on the lining membrane of the vein.

The aneurism had likewise pressed backwards and upwards upon the right bronchus, upon which, when distended by the contraction of the left ventricle, it must have made intermittent pressure; hence the feeble character of the respiration on the right side.

This case seemed to him to be of considerable interest in many points of view. In the first place, the physical evidence for a positive diagnosis was imperfect. The diagnosis of aneurism was made rather from the general symptoms and history of the case than from physical evidence. There could be no doubt, from the engorgment of the superior vena cava and its tributaries, that pressure was exercised upon this vessel. This pressure was manifestly due to a tumour, which likewise, in all probability, in the absence of evidence of disease of the right lung to explain the feeble character of the respiration on that side, made pressure on the right bronchus. From the history of the case, as well as from the physical signs such as they were, there could be little doubt that this tumour was aneurismal. Thus there was a double bruit at the base of the heart. One of these became more intense as it was traced upwards, and at the right of the mesial line of the sternum it attained its maximum of intensity. At this situation, likewise at the acme of expiration, there was a very feeble impulse and thrill experienced. The physical signs, though not of a very decisive character, went strongly to confirm the

diagnosis of aneurism of the aorta. The absence of well-marked impulse, positive dulness, and dysphagia, was explained by the position of the aneurism with relation to the lungs and œsophagus.

There were several cases of this kind reported in the *Proceedings of the Pathological Society.*

The case which most closely resembled that now before the society was exhibited and described in 1842 by Professor Law. As in the present case, the physical evidence was very imperfect. There was no impulse or fremitus. Respiration afforded but little evidence. There was no dulness on percussion. In that case the tumour had formed on the posterior wall of the aorta and found entrance into the cava about an inch above the connexion of that vessel with the auricle. The appearance of the patient in that case was very like that of the man of whom he (Dr. Hayden) was now speaking. Most of the members present would recollect the remarkable case reported by the late Dr. Morgan in the November number of the *Dub. Quart. Journal* for 1853. The tint of the skin in that case was different from its colour in the present instance, being of a red crimson hue.

The interest of this case was considerable in other respects. For example, there was a double bruit over the base of the heart without aortic valve disease, this bruit being permanent. Hitherto it had been held by the Dublin school that a permanent double murmur over the base of the heart should be accepted as evidence of aortic valve disease; but here was a case of double bruit where the aortic valves were healthy. It has been conjectured that the growth of aortic aneurism arising within the pericardium as being due mainly to the reflux of blood in that vessel, is more likely to be in the direction of the heart, of which Professor Smith has recorded a remarkable example in the ninth volume of the *Dub. Quart. Journal.* To this rule the present case, as well as that of Dr. Law, constitute exceptions.

In the cases recorded by Drs. Law and Mayne the pressure on the cava was made above the vena azygos. Here it was below it; and this must make a difference as regards the state of turgescence of the vessels. Here the azygos, being pervious, was capable of carrying on the circulation to a certain extent, and was therefore greatly enlarged; whereas in those cases where pressure was exerted above the azygos, it could not aid in carrying on the circulation save by collateral channels of communication from the internal mammary and intecostal veins.—*December* 16, 1865.

Cystic Disease of the Kidneys.—Dr. G. F. Duffey, Assistant-Surgeon 1st Battalion 24th Regiment, detailed the following remarkable case, and exhibited the specimen and an illustrative drawing:—

On the morning of the 3rd of November, 1865, I was asked to visit a

soldier's wife who was in labour. She was twenty-two years of age; this was her third pregnancy. The first child was living; but in the second pregnancy she had a miscarriage. The waters, which were not excessive in quantity, had come away during the night. On examination I found that the feet were presenting through the external orifice, with the toes towards the pubes, and the left leg a little in advance of its fellow. She had comparatively frequent but short pains. The bowels had been opened and the bladder emptied. Little progress being made, the uterus was excited to stronger action by friction, and during the pains gentle traction was made by the feet; but although there was considerable delay in the delivery of the breech, notwithstanding that the pains got better, the expulsion of the abdomen was attended with great difficulty, and eventually was not effected without slight laceration of the perineum. The head and upper extremities came away simultaneously with the body. When the feet were touched, during the time they protruded through the vagina, they retracted quickly; but when the child was born it did not respire. The placenta came away in about fifteen minutes and was apparently healthy.

On examining the fetus, which was of the full time, I observed that it was anencephalous. Its face had that hideous appearance which the French term " Tête de crapaud;" the appropriateness of this description was borne out by the midwife, who exclaimed that it was like a toad; its neck and chest were very short, and as it were fused into one piece. The bones of the vault of the cranium were undeveloped, and the integument of the face and neck was continuous with a reddish-coloured structure, composed chiefly of areolar tissue, which immediately covered the rudimentary encephalic ganglia lying on the base of the skull, to which ganglia was connected a perfect medulla oblongata and spinal cord. There were curious malformations of the hands and right foot; the fourth and fifth fingers of the right hand were united by a web up to the distal end of their second phalanges, and there was an additional or sixth finger, with a distinct nail, similarly united to the entire length of the fifth finger. The left hand and right foot had corresponding malformations, viz., an intimate union of their respective third and fourth digits, which appeared as one, but had each separate nails. The left foot was normal.

The abdomen was greatly distended, of a globular form, and dull on percussion; it measured eighteen inches in circumference. On laying open the peritoneum two large tumours were found, one on either side, almost entirely filling the abdominal cavity. The liver was pushed upwards, and the intestines greatly displaced. The stomach could not be then seen. On examination these enormous tumours, to which the size of the abdomen was owing, were found to be the kidneys; and lying on and adherent to their surfaces were the ovaries and Fallopian tubes,

which led to a bifid uterus. A small gelatiniform polypus, attached by a long filamentous peduncle to the labium minus of the left side, hung down between the œdematous lips of the vagina. The bladder was empty. The supra-renal capsules appeared normal. There was no fluid in the bladder. On removing the capsules from the kidneys their surfaces presented the usual lobulated appearance of the fetal organ, but on making a section they were found to be composed of numerous small transparent cysts, visible to the naked eye, of about the size of peas, lying in a matrix of a light greyish colour, from which they could not be detached, and containing a clear serous fluid: the distinction between the cortical and medullary portions was totally obliterated, but the outline of the calyces could be indistinctly traced; the ureters were pervious. In consequence of the wish to have a drawing of the parts taken *in situ*, the kidneys could not be weighed until after their section and exposure to air for some days; even then the weight of the right kidney (both being apparently of the same size) was 6 oz; in length it measured $6\frac{3}{4}$ inches, in width 5 inches, and in thickness 2 inches. The fetus itself weighed 90 ounces; the proportion, therefore, of the weight of the kidneys to that of the entire body would be about 1 to 75, instead of, as in the normal fetus, 1 to 80.

This case, as well as being illustrative of the independent liability of the fetus to disease, is also, if one may judge from the scanty literature on the subject, interesting as an example of a remarkably rare degeneration—interesting alike to pathologists and accoucheurs. Not laying much stress on the anencephalic condition of the fetus, except as regards its connexion with hyper-development in other parts, which I think is unique in this case, I may mention that there are but few examples on record of a similar degeneration. Rayer, in his splendid plates, delineates a case of *atrophy* and cystic degeneration of *one* kidney in a fetus. In this case the cysts were larger, but not so numerous as in mine, and the other kidney was healthy. In Vol. III., p. 514 of his book, he states that examples of this degeneration of the kidneys of newly-born infants are very rare, and he only alludes to three somewhat similar cases. Virchow also, in a paper on "Congenital Dropsy of the Kidneys" (*Verhandl. der Phys. Med. Gesel., in Wurzburg*, 1855), relates some instances of cystoid degeneration with complete atresia of the pelvis and papillæ, and refers to this affection as a cause of obstruction to labour. In a case reported by M. Siebold, of Gottingen, in the *Rev. Med. Chir. Janvier*, 1855, the head presented, but the expulsion of the child was arrested by the size of its abdomen. Repeated tractions were required. The circumference of the abdomen was seventeen inches. On inspection two enormous tumours were found in the abdomen; these were the kidneys—the two weighed *two pounds*. Each was six inches long, four wide, and three thick. On removing the capsule,

small cysts, containing a transparent serosity, were seen scattered about. On section the cortical substance could not be distinguished from the pyramids, the greater portion consisting of small cysts visible to the naked eye. In Virchow's and Siebold's cases, and I think in mine also, these cysts were dilatations of the urinary canals.—*December* 16, 1865.

Diphtheria.—Dr. Fleming said that on the 24th December, a young man, having all the outward characters of good bodily health, presented himself at the Richmond Hospital, labouring under what he described as lumps in the throat, accompanied by a sensation of choking. He was immediately seen by the resident pupil, Mr. Bredon, then on duty, and placed in bed, and it was remarked that his respiration was very rapid, that he was in a state of extreme feebleness and debility, and that death was rapidly impending. After a short time he was able to give some little account of the nature of his illness, and made a statement which was afterwards confirmed by his friends. About ten days previous to his application at the Hospital he was attacked with what he called ordinary sore throat. He applied to a dispensary in the neighbourhood where he lived—Parkgate-street, a very healthy neighbourhood—and got some medicine, which he carelessly took. He continued at his work for a week, as porter at the railway adjoining. Shortly after his admission he (Dr. Fleming) happened to be at the Hospital, and was brought to see him, and at that moment he presented all the features that would have indicated in a child croup in an aggravated form. He examined his fauces carefully, and there was an absence of the slightest deviation from a healthy condition, or the slightest fetor of the breath. As well as he could he examined the man's chest; it was impossible to put him with safety into a sitting posture for more than a minute or so; but Mr. Bredon and himself (Dr. Fleming) found that there were râles over the base of both lungs. His pulse was barely traceable, the respiration very rapid, and he was bathed in perspiration. The usual treatment was recommended, and he contemplated having the assistance of some of his colleagues, with a view to any operative interference, when he was suddenly sent for by one of the nurses, who told him the man was dying. He found a very serious change in his symptoms, bad as they were at the time of admission. His countenance presented a ghastly appearance, bathed in perspiration, and with a livid hue over it; the lips were also livid, and the man was pulseless, and gave a sort of jerk in breathing that appeared to be extremely distressing. At this time the neck was not extended, as was generally the case in croup, but was somewhat flexed. He thought at the moment it was advisable to open the larynx, and he did so. The operation was rapidly performed, not a drop of blood was lost, and the tube was introduced without any difficulty.

He was immediately attacked with a sort of convulsive fit of coughing as was usual under such circumstances, and threw up through the tube and mouth a quantity of semi-purulent fluid, slightly tinged with blood. Every two or three minutes he had a convulsive fit, and the fatal termination of the case was evident. Every attempt was made to induce artificial breathing, and the resident pupil, Mr. Bredon, did not hesitate, with that view, to have recourse to a measure sometimes adopted in such cases. Nothing however succeeded, and the poor man died a few minutes after the operation. The history which he gave of his case was confirmed by his friends. He lived with four or five other young men, all of whom were healthy, in the same room; there was no epidemic either of scarlatina or diphtheria in the neighbourhood. Dr. Fleming then exhibited a drawing of the parts as they appeared on a *post mortem* examination, the drawing executed by Mr. Connolly being described as a very accurate delineation of the appearances which were presented. It would be seen, continued Dr. Fleming, that the back of the epiglottis, the inside of the larynx where the vocal cords appeared to be obstructed, and the whole side of the trachea extending into the bronchial tubes, were lined with a membrane tinged with a larger amount of greenish hue than was commonly the case. There was not the slightest firm attachment of any kind between this membrane and the lining membrane of the tubes, but there was a great amount of vascularity existing. The preparation had lost the characteristic appearances marked on the drawing, but yet the membranes were distinctly traceable throughout the whole canal of the trachea and its bifurcations.

As to the nature of this disease there must to a certain extent be some doubt. One might consider it diphtheritic laryngitis or diphtheritic tracheitis. He would not say there were not cases of disease of this kind confined to the larynx and the trachea, but in most of the cases he had seen the fauces engaged, while in the present case nothing of the kind existed. There was no indication of swelling of the neck, the glands were free from enlargement, and the surface of the body exhibited no eruption of any kind indicative of any constitutional affection. Some years ago when he had charge of a medical ward in Steevens' Hospital, he recollected the case of a young man, about twenty years of age, who was in a convalescent state after fever, when he was suddenly attacked with symptoms of croup. Operation was deemed not advisable. He thought Sir Henry Marsh exhibited the specimen at one of the meetings of the society, as illustrative of the condition that existed in this disease. The specimen and drawing much resembled those now exhibited. In the present case the opening of the larynx was effected with ease, and there was no doubt the tube passed within the circle of this morbid exudation. In many instances it had happened in passing the tube, either in tracheotomy or larynotomy, that it passed between the lining membrane of the larynx

or trachea and the abnormal membranes. The case was a most interesting and important one. The opinion of Trousseau as to the advisibility of operation in such cases was different from that entertained by surgeons in this country; but it was probable, however, that the cases Trousseau had met with were different from those we were accustomed to here. The lungs presented the appearance usually found in cases of laryngeal or tracheal obstruction.—*January* 13, 1866.

Hypertrophy of the Heart, Cirrhosis of the Liver, &c.—Dr. Hayden submitted to the society the diseased viscera taken from a man who died within the last few days, of general dropsy. He was aged 44; was a discharged soldier of intemperate habits, and had been several years in India. About four months ago he reported that he observed, for the first time, an unusual frequency in passing water. A month subsequently he noticed a swelling of the ankles, and shortly afterwards of the lower extremities generally, and of the abdomen. He came under Dr. Hayden's notice for the first time on the 8th of January. He was then generally dropsical; the lower extremities and genitals were very much distended, the peritoneal cavity was full of fluid; and the intestines distended with flatus; ramifying over the left side of the abdomen were several large, tortuous, and distended veins, the colour of which contrasted strongly with that of the skin, which was of waxy whiteness. The face was likewise œdematous, as were the arms and chest; respiration was very much embarrassed; the man's pulse was quick, but regular, and rather strong. The heart pulsated with considerable force in the usual situation. Its sounds and its rythm were strictly normal; the precordial dulness was extended. The man was scarcely able to lie down so embarrassed was his breathing. The urine was found on examination to contain albumen in large quantity; its specific gravity was 1010, reaction acid; the precipitate, examined microscopically, exhibited tube-casts of two kinds, viz., some waxy or hyaline, without epithelium, and others of a similar character, but studded with fat granules *vice* epithelium. Respiration was loud on both sides, except posteriorly, and inferiorly on the right side, where it was feeble, and accompanied with râles; here, there was dulness, but elsewhere percussion seemed normal. There could be no doubt whatever that there was disease of the kidneys, and it was equally evident that there was disease of the liver; because the effusion into the peritoneum was out of proportion to the dropsical effusion elsewhere, and further evidence to this effect was afforded by the presence of distended veins on the abdomen, showing visceral obstruction. But still further it was manifest from the great extent of precordial dulness and from the strong action of the heart, that there was hypertrophy of that organ. The idea of valvular disease was negatived by the absence of murmur and irregularity in the heart's action. An

attempt was made to relieve the man by means of an acute purgation with elaterium, but the stomach refused to tolerate the medicine. On the night of the 10th the man vomited up a large quantity of dark discoloured blood. On the following morning he (Dr. Hayden) found him breathing stertorously; pulse irregular and intermittent, and the impulse of the heart was not to be felt. It was manifest that effusion was taking place into all the serous cavities, amongst others the pericardium. The man was quite conscious. He attempted to sit up in bed and to speak, and whilst making this effort died in a state of syncope.

On examining the body after death, an immense accumulation of fluid was found. The liver was cirrhosed; it was lobulated on the surface, and mapped out into small spaces by the thickened capulse of Glisson. The peritoneal investment of the liver was likewise thickened; the organ was contracted in its transverse, and likewise in its vertical diameter; it was increased in diameter antero-posteriorly. On section it did not show an advanced stage of cirrhosis, which accorded with the fact that there had been no olive discolouration of the skin at any time. The spleen was found to be considerably enlarged, and its capulse was thickened and opaque over a space about the size of a crown-piece; this portion of the capsule, which was about one-eighth of an inch thick, projected into the substance of the organ, and by this the spleen was attached to the diaphragm. The splenic artery was dilated, and remarkably tortuous, nearly as large as the arteria innominata; the left kidney was rather larger than this organ was found to be in health. On section it presented rather a mottled appearance. The cortex could not be said to be diminished, it was rather increased in thickness. The Malpighian pyramids were large; a section of the cortical substance examined microscopically showed fatty disease in an advanced stage.

The right kidney resembled the left in general appearance, but was very much smaller; it did not much exceed in size the supra-renal capsule of the fetus.

The heart, as was anticipated, was found to be hypertrophied on the left side. The right auricle and ventricle were rather dilated, as was likewise the right auriculo-ventricular opening; so much so, that four fingers might be made to pass through it with great facility. The left auricle was rather thicker in its walls than in health. The left ventricle was concentrically hypertrophied; the walls being considerably over an inch in thickness in the mid-region, and towards the apex somewhat less. This cavity was contracted; the valves were healthy both on the right and left side. If it be admitted that the left ventricle was contracted in size, the absence of valvular disease was a circumstance of interest. The pleural cavities were full of fluid. The right lung sank in water, the left floated, but he was able to inflate the right with great facility, and then it floated also. This man died certainly of effusion, and not of uremic

intoxication. The features of greatest interest were—first, the hypertrophy of the left cavities of the heart, affording a good example of that condition of the heart which Dr. Kirkes pointed out as frequently associated with Bright's disease; secondly, the small size of the left kidney; and thirdly, the condition of the liver and of the spleen.

The following measurements were taken January, 16th, *i.e.*, five days after the patient's death, when, it is to be presumed, *rigor cordis* had departed, and after much and forcible stretching of the ventricular walls.

Walls of left ventricle measured in thickness, at apex, seven-eighths of an inch; at middle portion, one inch and one-sixth; at base on level of free extremities of mitral valve, one inch and one-third.

Length of cavity of left ventricle from aortic valves to apex, three inches.

Width of cavity of left ventricle from septum in middle of cavity, one inch and a half.

Thickness of left auricle in central portion, one-eighth of an inch.

Thickness of right ventricle at apex, one-sixth of an inch. Thickness in middle portion, one-fourth of an inch. Thickness at base on level of free extremities of tricuspid valve, one-eighth of an inch.

Circumference of entire heart at base of ventricles, eleven inches and three-eighths.

Circumference of left ventricle at base, ten inches and one-eighth.

Circumference of aorta two inches above valves, three inches and one-sixteenth.—*January* 13, 1866.

Gangrene of the Œsophagus.—Dr. Lyons brought under the notice of the society a case of gangrene of the œsophagus, arising from a somewhat unusual cause. The man, who was the subject of it, was at breakfast on Sunday week last, and being, as he described it, exceedingly hungry, eating bread and some fried liver, he bolted a large piece of crust of bread which he described as being of the dimensions of his fore finger with a portion of liver along with it. This stuck in his throat, and he made violent efforts by passing his finger in as far as he could, and by repeated regurgitating efforts to dislodge the impacted piece of bread. This he believed he succeeded in doing, and for the remainder of the day he suffered little or no inconvenience; neither did he suffer any inconvenience on the Monday or Tuesday following. On Wednesday he was admitted to Jervis-street Hospital, complaining of a certain amount of uneasiness in the throat. There was a great deal of swelling below the angle of the jaw on the right side, with some tenderness in that situation. He (Dr. Lyons) made a most careful examination of the throat; a burst of strong sun-light which occurred at the moment enabling him to do so satisfactorily. He passed his finger as far down as he could into the throat, felt the upper portion of the larynx very

distinctly, and got his finger a short distance below it, but failed to find anything. Owing to the swelling, and tension, and pain that existed, a few leeches were applied to the throat, and poultices subsequently, and the next day the patient found himself considerably relieved. He seemed to be progressing tolerably well, the swelling was considerably reduced, and he was up, and on last Sunday he (Dr. Lyons) found him sitting at a table in the ward eating a portion of a mutton chop, and he was informed that the man eat one-third of a mutton chop on that day. On Tuesday he requested permission to leave the hospital for a short time, and was out some three or four hours. He returned perfectly sober, and on the next day complained of a considerable degree of uneasiness. Still he was able to swallow fluids, arrow-root, beef tea, wine, and some spirits and water, which he got during the night, on one occasion, when he felt faint. On Tuesday evening a gargle was given him; and in the effort to gargle he brought up a quart of foul, fetid, grumous matter, partly mixed with blood. He (Dr. Lyons) saw him the next day and again carefully examined the throat, but was unable to ascertain the presence of any foreign body. On Wednesday night between six and seven o'clock he took a cup of arrow-root, which he swallowed after some effort, taking it slowly in spoonfuls; and about one o'clock next morning was seized suddenly with an attack of weakness, and threw up a quantity of grumous stuff. Dr. M'Donnell, one of his colleagues, being the accident surgeon on duty, was sent for, and, just as he arrived, the man expired.

A *post mortem* examination was made. The tongue and all the back parts to the spine were removed, and there was found upon the spine evidences of most extensive gangrenous destruction of all the tissues about the œsophagus, extending from the lower part of the pharynx down a long way in the posterior mediastinum, and near to the cardiac orifice of the stomach. He now proceeded to slit open the œsophagus, and found on doing so an obstruction in the shape of a thin piece of bone lying right across the tube. The drawing made by Mr. Connolly, which he now exhibited, showed accurately the position that it occupied. It was from an inch and three-quarters to two inches below the level of the rima glottidis, and it was impossible it could be touched with the finger, or that any physical evidence of its presence could be ascertained by digital or ocular examination. It appeared to him to be a sharp piece of bone, such as would be detached from a rib of bacon. It was curious that the man repeatedly said he was breakfasting on bread and liver, and made no reference to any bony substance, nor had he any suspicion that there was a bone or other substance in the throat, nor did he conceive, after he came into hospital, that there was anything in his throat, and he attributed the pain and uneasiness which he suffered to the violent regurgitating efforts and the violent manipulation which

he had used to get rid of the piece of bread and liver which constituted, in his own opinion, the obstruction in the first instance.

Here, then, was an example of gangrene of the œsophagus, which, as far as he knew, was very unusual from such a cause. The question arose what could have been done supposing they knew that there was a foreign body in the throat? It did not appear to him that operative interference was desirable. He did not see how any mode of procedure could be applied to detach the portion of bone without doing a considerable amount of injury to the soft parts in which it was imbedded; and as to the use of the probang, he congratulated himself on not having used it; for the probang meeting this portion of bone would push it further down, tearing and lacerating the mucous membrane, and causing further destruction of tissue and possibly extensive hemorrhage, and he should be inclined to blame himself for having caused so much injury and so large an amount of mischief consequent on the destruction of mucous membrane. Bodies impacted in the throat frequently found their way into the stomach, and were then passed *per anum*. Sir Benjamin Brodie spoke with hesitation as to any operative proceeding to extract bodies that could not be felt. He said:—"But let us suppose that the foreign body cannot be felt with the finger, are you then to attempt to take hold of it with the forceps? Really, to extract a foreign body from the œsophagus, below the part at which you can feel it with the finger would be a very difficult operation, and probably not a very safe one; for, in poking with the forceps, you might carry them through the coats of the œsophagus."—Works of Brodie, Vol. III., p. 513.

In this particular instance, where a piece of bone was so very sharp on both sides, it would have created a vast amount of laceration had it been forcibly pushed down. Again, the mere fact that a patient thinks there is a foreign body in his throat is no proof that it is there. Sir Benjamin Brodie, on this point, says:—"There is another matter of considerable practical importance, to which I wish to call your attention, with respect to matters supposed to be stuck in the œsophagus. A woman was brought to town who was thought to have swallowed a piece of bone, and I believe that there was no doubt that she had done so. I introduced my finger, and, not being able to feel it, I concluded that it was below the reach of the finger. I then passed an œsophagus bougie into the stomach, but could not feel it. I then introduced a probang with sponge, but with no better effect; but still the woman had the sensation of its being there. I now began to doubt whether it really stuck there, and to suspect that the sensations that she complained of indicated that some part of the œsophagus had been abraded or torn by the foreign body, but that the body itself had passed into the stomach. It is a common trick with conjurors to put half-a-crown into the hands of a person, to press it firmly, and then to say to him, 'You are sure it

is there?' The party says 'Yes.' In fact he has the feeling of it, but when he opens his hand it is not there."—*Ibid*, p. 517.

It is further worthy of note that the probang has been found, in more than one instance, to pass freely into the œsophagus without detecting a foreign body when somewhat of the dimensions of that in the present case, the instrument passing either in front of or behind the obstruction. Such was the case in an instance which occurred in Jervis-street Hospital, in the practice of a gentleman now deceased.

He thought this case worthy of being brought before the society for several reasons. First of all as an instance of gangrene of the œsophagus arising from an unusual cause. Gangrenous destruction of the œsophagus from swallowing caustic fluids they had examples of; and in the Museum of the Richmond Hospital there were drawings of cases where it had arisen from swallowing sulphuric acid, but he was not aware of any case where a foreign body impacted in the throat had led to such destruction of the soft parts. Again, this case showed that surgeons should not rely too implicitly on the statements of patients as to what they swallowed. It could not well be conceived how a man taking such a breakfast as bread and fried liver could have swallowed a portion of bone, and that without being conscious of it.—*January* 13, 1866.

Cerebro-Spinal Arachnitis, with Paralysis of the Right Third Nerve.—Dr. Banks said that on the 10th of October, 1865, a girl, aged twenty, was admitted into the Hardwicke Hospital. She stated that, four days before, she had caught cold, and had two or three attacks of shivering, followed by general uneasiness and pain in her back, and so ill did she feel that she immediately took to bed. On the fourth day she was received into hospital. The symptoms she laboured under were those generally met with in the first four or five days of simple continued fever. Her skin was hot and dry, her pulse 90. She had some slight headache and general uneasiness, and her bowels were constipated.

For three days after admission her symptoms did not undergo any remarkable change. The temperature rose, as ascertained by the thermometer; the pulse rose in frequency, and she had more headache, and her sleep was very disturbed, and there was some slight wandering. On the fourth morning after admission, and the eighth day of her illness, the resident pupil, Mr. Little, who observed the case very closely, ascertained from the nurse that a very extraordinary change had taken place the preceding night. It appeared that her mother had paid her a visit; an altercation arose, which ended in a struggle, and the mother by force removed a little bag of money which was about the girl's neck. After this she became violently delirious, could with difficulty be kept in bed, and raved continually about money. The pulse was more rapid, the temperature had risen, the head was much hotter—in short, there was

marked exacerbation of all the febrile symptoms. For the next three days she was violently delirious, she disliked the light, and her hearing became morbidly acute. She could repeat conversations carried on at the very end of a large ward. On the twelfth day of her illness for the first time a new train of symptoms presented themselves. She was observed, as she lay on her back, to have her head thrown very far back. She could not bear the lightest pressure on the neck, chest, or abdomen. The slightest movement, moving the arm or leg, produced extreme pain, although at the same time she was flinging about her arms and legs in a very violent and excited manner. The delirium continued, the pulse rose in frequency still, as did the temperature, and she spent two or three nights almost sleepless. The character of the delirium resembled that in delirium tremens; it was a busy sort of delirium; she was more engaged about her own sensations than raving about the money which for the first two or three days after the fight with her mother was the only subject of her wanderings. On the fifteenth day of her illness it was observed that there was ptosis of the right eye and great dilation of the pupil, with *divergent* strabismus. She had scarcely any cough up to this time, and the respiration was not more rapid than would be expected from the rapidity of the circulation, but on this day her respiration became extremely embarrassed, and on examination of the chest signs of universal bronchitis were discovered; there was no dulness on percussion. She sank gradually, and for some hours before death she manifested symptoms of death by the chest rather than by the head. The face and extremities became livid, and there were all the marks of malaëration of the blood; and on the 18th she died.

An examination revealed the following morbid appearances:—The cerebral sinuses and the vessels of the pia mater were much distended with dark blood. The upper and lateral surfaces of the brain did not present anything abnormal. The base of the brain was the seat of extensive deposition of lymph of a greenish yellow colour, which had caused it to contract adhesions, particularly to the lesser sphenoidal wing. This deposit commenced in front, a few lines beyond the optic commissure, enveloping it completely, also covering and concealing the roots of the first nerves and closing up the entrance of the fissure of Sylvius. From thence proceeding backwards it completely covered and concealed all the parts situated in the interpeduncular space, and enveloped, for a considerable distance, both crura cerebri. Over the pons varolii it was very thick, diminishing towards its posterior edge, and the arachnoid covering the medulla oblongata was merely thick and opaque. The deposit was more abundant, and the layer was thicker, on the right than on the left side, especially where it covered the crus cerebri and the pons.

The third pair of nerves, in common with the other parts contained in

the interpeduncular space, were enveloped in lymph; but on examination it was found that the right nerve had undergone a remarkable diminution in size, when compared with its fellow on the other side. The right nerve appeared to have scarcely half the diameter of the left.

On opening the theca of the spinal cord the arachnoid was found thickened and opaque, with deposition of lymph over its entire extent. The amount of lymph varied in different situations; it was very slight on the medulla oblongata and upper cervical part, was thickest at the brachial enlargement and in the spinal region, but was distinct down to the very middle of the cauda equinæ. The lymph on the spinal arachnoid presented the same yellowish-green hue as at the base of the brain.

In the lower and anterior part of the lumber enlargement a small superficial effusion was found in the meshes of the pia mater.

The *lungs* were the seat of miliary tubercle, sparsely scattered everywhere except at the posterior parts, where they were rather closely set; the bronchial membrane was inflamed.

The *spleen* was large, and studded with tubercles, which were much more abundant than in the lungs.

The mucous membrane of the small intestines was the seat of inflammation, particularly the jejunum and the ilium, towards its termination; and the patches of Peyer were more distinct than in health; but there was not a trace of ulceration.

Dr. Banks observed that the pathological appearances in this case, so far as the brain and spinal marrow were concerned, resembled those which were observed in 1846, when cerebro-spinal arachnitis appeared in an epidemic form. Some of the symptoms were identical with those of that terrible disease, but in other respects it differed in a remarkable manner.

He particularly called attention to the duration of the disease, which contrasted with the fearful rapidity which was one of the most striking characteristics of the epidemic disease.—*January* 20, 1866.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.[a]

TWENTY-EIGHTH ANNUAL SESSION.

Dr. M'Clintock, President.

Remarks on Gynecomazia. By Dr. Foot.

Under the title of gynecomazia I wish to make a few remarks on the development of the mammary gland in the male. The term gynecomazia is not the coinage of modern days, it is a classical word employed in the second century of the Christian era by Galen, in his four hundred and third definition, to signify an unnatural development of the fat underlying the breast, and is also alluded to by Paulus Egineta (Lib. VI., Sect. 46). The first time any attention was drawn to enlargement of the breast in the male subject was in 1863, when I saw a boy, fourteen years of age, with the mammary gland on the left side strikingly developed; the gland on the right side was of the usual size. There were no other appearances of effeminacy about him: he was stout and manly, well grown for his age, his voice was becoming deep, and the sexual hair appearing on his chin and cheeks. The breast had a firm glandular feel, hung downwards from its weight, was conical in shape, the nipple surrounded by a pale pink areola, studded with papillary projections. There had been no secretion observed from it. The development of the breast was discovered during the examination of his chest for other purposes: it gave him no pain or concern, and he was surprised at my wishing to take a cast of it. No attempt was made to reduce its size, as there was no indication for any interference with it. The next case I met with was in a man, between twenty and thirty years of age, when both breasts were equally developed, and had the external appearances of the virgin mammary gland He said a thin white watery liquid sometimes came from one nipple. I never saw this occur with him, and I tried in vain with a strong breast-pump to get any fluid from the lactiferous ducts. He was not unusually muscular, but the facial hair was present in abundance, and there was nothing feminine about him. In the next case there was a cystic tumour lying superficial to an enlarged mammary gland. It occurred in a lad of sixteen, of manly manner and appearance. He suffered pain from the cystic tumour, which had existed a year at the time I saw him, but had lately been getting larger, and becoming painful. The left breast was the one affected. The tumour was seated at the base of the nipple, was about the size of a large gooseberry, tense but fluctuating; no fluid had been discharged from it. His friends were alarmed at the pain and swelling of his breast, and made him get advice about it, much against

[a] These Reports are supplied by Dr. Geo. H. Kidd, Secretary to the Society.

his own inclination, as he seemed quite ashamed of having, what he thought, such a feminine attribute about him. The ointment of iodide of potassium applied locally, with protection of the breast from his shirt front by a layer of cotton wool—and the iodide of potassium internally, reduced the size of tumour, and lessened the pain; then tight strapping with adhesive plaster almost restored the gland to the size of the opposite one. There are reasons for believing that the use of the compounds of iodine is particularly applicable in such cases. There is nothing in the anatomy of the male mammary gland to forbid its assuming the form and functions of the female breast, though its parenchyma is scarce, appearing as a white fibrous mass, without showing any arrangement into lobules, and the glandular tissue displays only few of the vesicles so numerous in the female mamma; yet, both in man and in the lower animals, there are many instances in which the male breast has become enlarged and secreted milk most actively. It is well known to the members of this society that this tendency on the part of the male breast is especially marked in the infant a few days after birth, and again at the advent of puberty. Microscopic examination of a specimen of milk from the mammæ of a male child, nine days after birth, has shown the presence of numerous milk globules; and chemical analysis has confirmed the testimony of the microscope. In the museum of the late Dr. Montgomery was the breast of a man who had mammæ like a young woman. Bristles are passed through ducts in the nipple into the substance of the gland; the genital organs were well formed. He was forty-five years of age, had never married, and died of phthisis.—*Signs and Symptoms of Pregnancy*, p. 125.

In the conclusion of his third book of the *History of Animals*, Aristotle speaks of a male goat at Semnos which yielded milk enough for cheeses to be made from it, and whose male descendants inherited the peculiarity; and Isidore Geoffry St. Hilaire had for some years, in the Jardin des Plantes, a male goat with largely developed udders yielding milk (*Aristotle*, G. H. Lewes, p. 285). It is remarkable that the male mammary glands are seldom both enlarged, it is generally on the left side the anomaly exists. John Hunter gives brief notes of five cases of enlargement of one breast in men, from sixteen to twenty-seven years of age, and then cites the case of a man who nursed eight of his children from his left breast (*Essays and Observations*, edited by Prof. Owen, Vol. I., p. 238). In 1855 Dr. Tufnell (*Dub. Quart. Jour.*, Vol. XIX., p. 230) read before this society the particulars of the case of a soldier of the 60th Rifles, whose left breast was developed in form and size equal to that of a female at puberty—the nipple was prominent, the areola well marked, of a pink colour, and much brighter than that of the opposite side. The man's attention was first drawn to the enlargement of his breast when about twelve years of age. His sexual characteristics were fully marked.

There are many other recorded instances of this unilateral development. In the case of an athletic blacksmith whose left breast became, without cause, in six weeks, as large as that of a nursing female, and was filled with the lacteal secretion, soap plaster and compression reduced the gland to its natural size.—*Brit. For. Med. Ch. Rev.*, Oct., 1850, p. 551.

Pliny alludes to the African race called Androgynœ, whom the ancients reported to have a female breast on the left side, and a male one on the right.—*C. Plinii Hist. Nat.*, Lib. VII., Cap. 2.

The development of the mammary gland in the male may or may not be attended with an arrest of development of the genital organs. The testes are often observed to be small and soft when the male breasts are enlarged. In such cases it appears as if the adoption of one sexual character which does not properly belong to the male is compensated by the loss of another which does. The same thing is observed frequently in birds—hen pheasants, for instance, may partially adopt the voice and plumage of the male; and it has been pointed out that those hen-birds who do so are always affected with disease of the ovarium, and that those in which disease has made most progress have advanced furthest towards the male in external appearance (*Proc. Zool. Soc.*, Lond., 1860, Part 3, p. 373). In cases of disease of the ovary in domestic hens, the plumage remains perfectly unchanged, but the comb and wattles become extraordinarily developed, in many cases even surpassing those of the male bird in size. The birds crow like the males, and are popularly known as hen-cocks. The alteration has been observed as resulting from melanosis of the ovary, from cartilaginous degeneration, and from a generally diffused inflammation arising from the escape of an ovum from the oviduct.—*Op. cit.*, 1857, Part 25, p. 81.

Paulus Egineta, speaking of male breasts resembling the female, says, as at the season of puberty the breasts of females swell up, so in like manner those of the males also swell to a certain extent, but for the most part they subside again. In some cases, however, having acquired a beginning, they go on increasing, owing to the formation of fat below. Wherefore, as this deformity has the reproach of effeminacy, it is proper to operate upon it. He then proceeds to describe the operation for its removal.—*Paulus Egineta*, Vol. II., p. 334, *Syd. Soc. Trans.*

It is now known that the enlargement is not merely due to formation of fat lying between the gland and the pectoral muscle, but to a true development of an organ which was intended to remain in a rudimentary condition.—*February 10th*, 1866.

Dr. Telford exhibited a specimen, and read the notes of a Case of large ovarian tumour of three years' standing; subsequent ascites, paracentesis; death twelve hours after; autopsy.

Mary Howard, aged thirty-seven, married, mother of five children,

three years since perceived an enlargement in the left side of the abdomen. She did not, however, pay much attention to it till ten months after its appearance, when she was inconvenienced by its increasing size. She then sought advice at the Rotundo Hospital, when the enlargement was found to be due to an ovarian tumour originating on the left side. In other respects her health was very good, the menstrual function being quite regular. The tumour was quite movable, and in every way she seemed a very fit subject for ovariotomy, which was proposed, but owing to the advice of her friends, she refused to submit. She then left hospital, and shortly after conceived, in April, 1864, and was safely delivered, at the full time, of a healthy child on the 15th of January, 1865. Her convalesence was natural. The tumour now rapidly increased in size, causing her great inconvenience, and a good deal of dyspnea. She was readmitted to hospital on the 17th of January, 1866, just one year after her confinement, when she presented the following symptoms:—General appearance cachectic; pulse rapid and weak; breathing laboured and difficult. On examining the abdomen it was found to be enormously distended by a large tumour, which in some parts feels very firm and resisting, the remaining portion conveying a very distinct sense of fluctuation. On placing the hand over the abdomen, a peculiar crepitating crackling feel is conveyed, such as is found in cases of emphysema. The hand passes easily between the pubes and the tumour. On placing the hands on either side of the tumour it can be lifted up, and appears quite movable, gravitating to whatever side the patient lies on. It is distinctly multilocular, several prominent cysts being felt through the parietes; the uterus is prolapsed. She was treated by nutritious food, wine, and preparations of iron. The prolapsed uterus was replaced, and kept in position by a large pessary. Owing to the enormous size of the tumour, the cachectic condition of the patient, and the presence of some ascites, an operation was not deemed advisable. She then returned home On Wednesday, the 7th of March, she requested to be tapped, the ascites having greatly increased since her dismissal. We found her very weak and feeble, with very difficult breathing—in fact, I may say dying. However, as she thought the tapping might give her some relief, the operation was performed in the mesial line, midway between the pubes and umbilicus, when about twenty pints of a brownish fluid something resembling muddy beer, were removed. She gradually sank, and died twelve hours after the operation.

Autopsy.—Permission was obtained to examine the abdomen only. The surface of the body was not as much emaciated as one would expect from such a severe illness. On opening the abdomen, a large tumour protruded through the line of incision. A few small adhesions existed on the anterior of the tumour, which gave way readily; the superior and inferior portions seemed totally free, but the posterior was firmly adherent

to the omentum, several large vessels ramifying from one to the other. The abdominal surface of the peritoneum was studded over with a number of tuberculous-looking masses, and the liver was adherent to the peritoneum. The tumour was now removed; but unfortunately, from the small incision we were allowed to make, the pedicle was broken down in separating the adhesions. The right ovary was also removed, and though not much increased in size, found to contain a number of small cysts. On examining the tumour it was found to be multilocular, containing eighteen distinct cysts, some of them contained within the other; it weighed twenty pounds, and was thirty-five inches in its greatest circumference. On opening some of the cysts, they were found to contain a milky-looking fluid, of the consistence of thin starch. Different parts of the tumour, and its fluid contents, were examined by the microscope, but no evidence of malignant disease was discovered.

This case, I think, very well illustrates the difficulty the surgeon has to contend with when consulted as to the advisableness of an operation in cases of ovarian disease, for here, though the tumour was believed to be unadherent, the *post mortem* showed most extensive adhesions. Some of them may, perhaps, have been recent, but others were certainly of some standing, judging from the size of the blood-vessels which passed from one to the other. Another interesting point in this case is the fact of the patient becoming pregnant, and going to the full time, while she laboured under such a severe form of disease. The fact of a patient becoming pregnant may occur often enough in this disease, but abortion is the usual result.—*March 20th*, 1866.

A Specimen of Disease of the Ovary. Exhibited by Dr. DENHAM for Dr. Babbington, of Derry.

Maria S., aged forty-five, unmarried, had one child, more than twenty years since, admitted into Derry County Infirmary 26th of September, 1865. States she had enjoyed good health till about three weeks since, when she began to complain of her stomach being out of order and swelled occasionally. Has menstruated regularly till five weeks since.

On her admission she complained principally of gastric irritation. On examination, there was fulness, enlargement, and tenderness on pressure, in the region of the right ovary. Leeches were applied; a blister; iodide of mercury ointment; tincture of iodine, bi-chloride of mercury administered internally, also iodide of potash and bromide of potash. All treatment proved ineffectual; the swelling and fulness increased.

About second week of October the disease appeared to have extended to left side, and there were indistinct evidences of some slight fluctuation. She menstruated in the second week of October.

The swelling and distention of the abdomen increased rapidly, accompanied with constipation, vomiting, and considerable general prostration.

No very acute pain was complained of at any period of her illness, and even to the last a hopeful looking forward to recovery. The distention rapidly increased, and distinct fluctuation being evident in the right ovary. It was tapped on Wednesday, 15th November, and 110 ounces of thick tenacious yellow-brown fluid drawn off, but with difficulty, as a long silver catheter was passed through the canula, and evidently broke down several cysts.

This operation did afford temporary relief, but she sank, and died exhausted, without pain, on Monday, 20th November.

In presenting these morbid preparations to the society, through my friend Dr. Denham, I have no observations to make. The *post mortem* examination revealed a frightful amount of diseased structures. In the right ovary, in which the ovarian disease was first detected, and from which 110 ounces were removed, the disease appears to be the form described and delineated by Professor Simpson in his lecture in the *Medical Times and Gazette*, October 29th, 1859; while the ovarian tumour of the left side is produced, in same page, as a section of a colloid tumour of the ovary. There is some colloid disease, also, in right ovary. The liver was full of diseased deposits, and the internal surface of the peritoneum was studded with a granules of a similar nature.—*March 20th*, 1866.

Inflammation of the Bladder and Vagina. By Dr. Atthill.

There are no forms of disease productive of greater distress and suffering than those affections of the bladder which are met with not only in married but unmarried females; and yet, while far from being uncommon, they seem to have attracted but comparatively little notice. Dr. Churchill has an excellent chapter on "Reflex Irritation of the Bladder" in the last edition of his work on disease of woman, and the late Professor Montgomery read an interesting paper a few years ago on "The Sympathy between the Uterus and Bladder," before the Medical Society of the College of Physicians; but I do not remember having seen the subject noticed elsewhere. I therefore thought the following remarks, founded on cases recently under my observation, would not be deemed uninteresting.

In considering these affections, it is necessary to divide them into three classes, which, while presenting great similarity in their general symptoms, depend on very different causes, and require for their cure very different treatment. We have, then,

1st. Those in which inflammation of the mucous membrane of the bladder occurs as a primary disease, either of an acute or chronic character.

2nd. Those in which inflammation, always in a chronic form, exists

in the bladder, this inflammation being generally a secondary affection, depending on some unhealthy action in the vagina or uterus, but occasionally being a primary affection, the anomalous action in the uterus or vagina being secondary to it.

3rd. Those affections of a purely reflex nature, in which, while all the symptoms are referred to the bladder, that organ is perfectly healthy, they being entirely due to, and caused by, some irritation, morbid sensibility, or inflammatory action, in uterus or its appendage.

The first of these forms does not fall particularly within the province of the obstetric practitioner. Every physician and surgeon is familiar with its character and symptoms. I shall not, therefore, dwell on the subject, but merely mention a few particulars of one case, which was mainly remarkable from the large quantity of pus secreted by the bladder, with comparatively slight constitutional disturbance.

An unmarried lady, aged about forty-five, was seized with a rigor, after exposure to cold. When I saw her some hours subsequently, she presented the symptoms of an ordinary febrile attack. She also complained of very acute pain in the back, which distressed her greatly. On the following day there was no improvement; on the contrary, the pain in the back was worse, and there was also a sense of soreness above the pubes, which was increased by pressure. In addition to general treatment, linseed poultices and other local applications were had recourse to, but with very partial benefit. On the next day she was, if anything, worse, constant desire to micturate being now a prominent and distressing symptom. I shall not, however, weary you with details of ordinary treatment; it is sufficient to say that on the eighth day of her illness I observed pus in the urine, and that it continued to be secreted in large quantities for a considerable time. The exact quantity which was daily voided could not be accurately ascertained, as it was mixed with mucous and other deposits, but certainly it could not have been less than three ounces daily. Nevertheless the patient improved; the pus gradually diminished in quantity, she was convalescent in about a month, and has since that time, now more than a year ago, enjoyed excellent health.

Cases coming under the second head are by no means unfrequent. In them we have well-marked symptoms of irritation, if not of inflammation, of the bladder co-existing with some unhealthy state of the uterus or vagina, and sometimes of the ovaries also. The existence or non-existence of actual inflammation of the bladder being that which distinguishes these, from those in which the symptoms are due to mere sympathy or reflex irritation, the cases we now alluded to form a most important class, the treatment of which require special care, for we are apt to fall into error if, having detected disease in one set of organs sufficient, probably, to account for all the symptoms, we neglect to investigate the condition of the others, and it by no means always

happens that the cure of the one affection, even though it should have been the primary, will be followed by that of the other, unless special treatment be adopted for its relief. The following case will, I think, illustrate this view :—

About a year ago I was consulted by letter relative to the case of a young lady residing in the country. The statement I received was to the effect "that the patient had been an invalid for nearly eight years. Her illness first began with a feeling of weight and distress in the lower part of the abdomen. This was so great as to prevent her walking, and was accompanied by pain all over the lower portion of the abdomen, and in the back. There was also irritation and pain about the vulva, especially during micturition, which always caused considerable distress. After she had suffered in this way for about three months, she was attacked with acute pain in right side of the abdomen. This was followed by shivering fits, which came on at intervals of about a fortnight. After a time a tumour was detected in the right side. The rigors and attacks of pain continued, however, to recur at nearly regular intervals for about a year, when they ceased, and from that time her condition began gradually to improve. For the last six years she has been perfectly free from these distressing symptoms, and of late fancied that the tumour had rather decreased in size; her general health, also, became pretty good; but the pain and soreness across the abdomen, immediately above the pubes, still remained. This prevented her walking, or indeed making any exertion. She also constantly suffered from considerable uneasiness in her side; but that which caused her the greatest distress, and indeed made life a burden, was the incessant desire to micturate, she being seldom able to remain more than an hour, never more than an hour and a-half, without passing water, and this even when in bed."

As I declined to give any opinion, unless I had an opportunity of seeing and examining the case, the lady came to town, and placed herself under my care. I should mention that I also received a communication from her medical attendant, a gentleman who is in considerable practice in the country. He stated that "the tumour in Miss —— side puzzled him very much. To his knowledge it had existed for upwards of seven years. At one time he thought it had changed its position, having been low down in the inguinal region, whereas it now lay nearly under the ribs, he had never been able to detect any attachment to the other organs, and candidly admitted that he was unable to give an opinion as to its nature; but that by far the most distressing symptom in the case was the extraordinary irritation of the bladder and vagina from which this young lady suffered, and which of late had become much more severe—so much so that life was a burden to her. He had tried all kind of treatment, tonics, sedatives, alkalines, and acids, without benefit. Sometimes he thought that a tumour or calculus might exist in the

bladder; but as the symptoms were vague, and the patient a diffident unmarried woman, he had not considered himself justified in making a special examination." I found Miss —— to be a very thin delicate-looking young lady, aged about twenty-six or twenty-seven years. Her colour was good, nor was there any unhealthy look about her. I did not obtain much additional information from her replies to my questions, except that she had from time to time been under the care of several medical men (among the rest that of the late Mr. Rynd, who had blistered her side), without obtaining any relief. The tumour referred to was very easily felt immediately under the ribs, the upper extremities being quite close to the liver; indeed, I think when lying on her back it must have been in contact with that organ. It was about six inches in length, and probably nearly four inches in circumference. It was completely above the pelvis, and certainly was not ovarian.

Handling it did not cause much pain, though she stated that occasionally it was very tender to the touch. She also informed me that at the very commencement of her illness, and before the tumour was detected, she was conscious of a sensation as if something had given way in her side. The tumour was, however, evidently totally unconnected with the affection of the bladder. It was not in contact with it, and pressure, even though made forcibly downwards, did not cause any uneasiness in that organ; therefore, though its size and weight must have caused considerable distress, her sufferings could not arise from that cause.

On proceeding to examine the condition of the uterus, vagina, and bladder, I found the former to be normal in size, and apparently healthy, but the vagina was very tender to the touch, and its mucous membrane greatly congested. I introduced a silver catheter into the bladder, but it did not contain either calculus or tumour. The urine was pale, of low specific gravity, and contained a considerable deposit, consisting mainly of mucous, with a small quantity of pus mixed through it. The analysis of the urine was made by my friend Dr. Head. The presence of pus in the urine, taken into consideration with the whole history of the case, led me to conclude that this lady was suffering from chronic inflammation of the mucous membrane of the bladder; that the inflammation of the bladder was probably the primary disease, that of the vagina the secondary; that the attack, originally acute, having been overlooked or neglected, had assumed a chronic form, and now gave rise to the distress this patient suffered. Acting on this supposition, I proceeded to treat the case by injecting the bladder with a solution containing ten grains of nitrate of silver dissolved in two ounces distilled water, to which was added, at the moment of use, twenty drops of opium wine. This treatment was steadily persevered in, the bladder being injected every third day. When first employed this gave considerable pain, but after a little time ceased to cause any distress, and I was now able to increase the

strength of the solution, and used ten grains to the ounce of water. The result of this treatment was very striking—in less than a fortnight she was able to retain her urine for fully two hours and a-half during the day, and for more than three hours at the night. Before another fortnight elapsed she was still farther improved, and now was seldom disturbed more than once during the night, and in the daytime easily passed three hours without inconvenience or distress. But at this point improvement ceased, and I was disappointed at finding that the inflammation of the vagina had but slightly diminished; as this produced considerable distress, I, though with reluctance (the patient being unmarried), made use of the speculum, and applied a twenty-grain solution of nitrate of silver to the whole surface of the vagina, every portion of which was intensely congested. This treatment I repeated twice a week for some time, at the same time persevering with the injection to the bladder. At the end of two months from the time that I first saw her she returned home, her condition being in all respects greatly improved. She was much stronger, had gained strength, and, though still unable to walk any distance without distress, could spend several hours daily in the open air. She could, without inconvenience, retain urine for more than three hours, and was never disturbed more than once at night, and that generally at about 5 o'clock, a.m. I have heard from her repeatedly since. She continued to enjoy what she describes as "great comfort;" and in one letter says, "I hardly know myself so great is the relief I have experienced."

The treatment employed in this case was suggested to me by the late Dr. Hutton. He was treating in this way a lady who was suffering from a severe form of inflammation of the bladder, when ill-health compelled him to go abroad, and he advised me to continue it with her for some time longer, the patient having placed herself under my care. On that occasion he told me that it was the only treatment he had found of any benefit in such cases, but that it was necessary to continue it for a considerable time, and in severe cases for not less than three months; and the results which have followed its use in my hands fully confirm the truth of this opinion.

In the foregoing there was no doubt but that inflammation of the mucous membrane of the bladder existed; but in by far the greater number of instances which come under the observation of the obstetric practitioner such is not the case. The patient refers her symptoms to the bladder, but on investigation that organ proves to be perfectly healthy. Dr. Churchill is the only writer who has called special attention to this subject. Dr. Montgomery, in the paper already referred to, narrates the particulars of cases in which a marked sympathy existed between the uterus and bladder, but he seems to have overlooked the most important, because the most common, of all the causes which give rise to this most distressing affection which is termed by Dr.

Churchill "reflex irritation of the bladder"—I mean, inflammation of the vagina itself. We have all seen instances in which cancer of the uterus, or even simple ulceration of the cervix, gives rise to irritation of the bladder, but after all they are (at least so far as my own observations go) rare in comparison with the cases in which vaginitis, simple and uncomplicated, is the sole cause. The existence of vaginitis itself is often overlooked, and instances have occurred to me in which the patient was assured that there was nothing wrong with her, because the os uteri alone was looked at, and the vagina itself, though intensely inflamed, entirely forgotten. I do not now allude to acute inflammation of the vagina, which occasionally follows parturition, nor to the acute attacks which sometimes come on after injuries or excessive sexual intercourse, but to the chronic varieties, which are met with frequently in practice. They are, I think, more commonly seen in the weakly and delicate than in the robust female. Frequent desire to make water, and, in severe cases, constant straining to do so, pruritus, sometimes smarting under micturition, and, not unfrequently, pain above the pubes, are the most common symptoms. In married women the act of coition often gives pain, and when this is the case the inflammation will be found to have extended almost to the vulva. This last symptom will aid in distinguishing vaginitis from ulceration of the os or cervix uteri, in which it is seldom observed, especially if it be of a cancerous nature. I lately saw a patient in whom nearly the entire of the cervix had been destroyed by cancerous ulceration, yet she did not suffer from irritation of the bladder, and sexual intercourse was painless. An examination of the vagina with the speculum, in patients suffering from vaginitis, generally gives pain; but a speculum must be used, for it is essential not only to see the condition of the mucous membrane of the vagina, but to apply our remedies through it. There is no occasion, however, to use a large instrument; a small speculum will answer equally well, and give much less distress to the patient. The vagina, on examination, will be found to present either an uniform deep red tint, or else to be covered with white aphthous looking patches, or even sometimes with a herpetic eruption. In either case a cure of the local disease will be generally affected by the application of a solution of nitrate of silver, joined to the use of a cold or tepid douche twice a day, and a tonic treatment; purgatives, except in cases of plethoric women, are to be avoided. Some patients will bear the application of a thirty or forty grain solution of nitrate of silver without its causing pain, but sometimes even a weak solution produces much suffering, and in one instance, that of a delicate married woman, I was obliged to discontinue its use altogether. This fact suggested to my mind the idea of applying, in similar instances, the caustic solution in the shape of spray, by means of one of "Maunders' spray producers," and though I have only used it in a couple of cases, I think it well worth a trial. You can apply the

spray through a speculum equally and evenly to all parts of the vagina, without irritating the inflamed surface by the contact of a rough brush or piece of lint. Rest in the horizontal position, if you can get the patient to carry it out, is, in severe cases, of great benefit. As the inflammation of the vagina subsides, the irritation of the bladder diminishes; and, if it depend solely on that cause, will probably entirely disappear. Vaginitis, though more frequent in married women, is not unfrequently met with in the single. I have seen it in three patients who were undoubtedly virgins, one of whom, a strong, healthy looking country girl, suffered so intensely from irritation of the bladder that life was a burden to her; yet the bladder, in her case, was perfectly healthy. In addition to vaginitis, reflex irritation of the bladder may be caused by inflammation, erosion, or ulceration of the cervix uteri, or by that condition of the canal of the cervix which gives rise to uterine leucorrhea. There is also one other affection which I have noticed as causing it—I mean simple hypertrophy of the uterus. This, which is not very common, seldom benefits much from treatment. It is, I think, generally met with in women who have reached middle life, and are otherwise healthy. It appears to me to act merely mechanically. The enlarged uterus pressing against the neck of the bladder irritates it, as it sometimes does in the very earliest stage of pregnaney. I recently saw an instance of this in a lady who has not long returned from India, where she had resided for several years. She had never been pregnant. The uterus, otherwise healthy, was much enlarged, and she suffered considerably from irritation of the bladder, especially during the early part of the day. I have also seen very similar cases in other patients who have resided in India, and I cannot help thinking that the habit of taking riding exercise, which is very common there, and sometimes even, as this lady acknowledged, during the menstrual period, must, by determining an undue amount of blood to the uterus, predispose to hypertrophy of that organ.—14*th August*, 1866.

TRANSACTIONS OF THE COUNTY AND CITY OF CORK MEDICAL AND SURGICAL SOCIETY.[a]

SESSION 1864-65.

PRESIDENT in the Chair.

On the Treatment of Pneumonia. By PROFESSOR O'CONNOR, M.D.

Within the last month Dr. Hughes Bennett has circulated, amongst the physicians in attendance on hospitals within these kingdoms, a

[a] These reports are supplied by Dr. Curtis, Secretary to the Society.

pamphlet having for its object to induce them to unite in determining, by statistical returns, the treatment most successful in the cure of pneumonia. He furnishes in this pamphlet forms which he wishes to be adopted in making the reports, and hopes that the result may be a final settlement of this disputed question by the British Association, to which these returns are to be submitted at its next meeting. Nobody can doubt the benevolence of his purpose; and if there was a hope that the scandal arising from warm discussions amongst physicians could be removed in this manner we should hail it as a great blessing. Unfortunately for me I am not sanguine about the result, but still I will humbly co-operate with the enlightened and benevolent purpose of its originator. When all these returns shall be collected they will have been furnished by men of different capacities for observation, different amounts of industry, prejudices produced by temperament, association, and habits; the cases themselves differing in circumstances of age, constitution, sex, previous habits, locality, type of disease, and with different complications, accidental or consecutive. They will then very much resemble the leaves in the Sybilline cave, which would require a sybil to reduce them to intelligible order. Dr. Bennett's treatment is what he calls the restorative plan, in contradistinction to the stimulant on the one side and the antiphlogistic on the other—the Scylla and Charybdis of medical therapeutics. If your patient is to escape being wrecked on brandy or sunk in the whirlpool of starvation and bleeding, he must seek the middle course of eight ounces of port wine, with beef-steak and chops. The treatment, in Dr. Bennett's own words, consists of salines, small doses of acetate of ammonia, with $\frac{1}{24}$-grain of tartar emetic, diuretics, spirit ætheris nitros, and træ cholchici. Nutriments—beef-tea and milk, taken early, with beef-steak, mutton chops, and eggs as soon as they could be eaten by the patient. The words are, as soon as they could be eaten by the patient; I think it would be better to have said as soon as they could be digested. People in the humbler classes of life would eat meat at the peril of their lives—as one of the good things they could never easily obtain. A physician is in charge of his patient in every respect, and is bound to supply the place of his lost judgment, and, perhaps, depraved instinct. I confess, as a rule, I would much like to see the pneumonia out of doors before I saw the beef-steak come in; which would require very pungent sauce to make it palatable where there is a loaded tongue, quick breathing, and a fevered brain. Dr. Bennett proves the propriety of his treatment, first, by its success as compared with every other system of treatment; his mortality being *nil* in 125 cases, whereas the treatment by bleeding ranged from $1{\cdot}7\frac{1}{2}$ to $1{\cdot}28$; by tartar emetic from $1{\cdot}4\frac{1}{2}$ to $1{\cdot}5\frac{1}{2}$; by diet alone from $1{\cdot}7\frac{1}{2}$ to $1{\cdot}15$; by stimulants, as by Dr. Todd, $1{\cdot}9$. And next he enters into an investigation of the pathological changes which take place in pneumonia, from which he deduces, as a necessary consequence, that his treatment is the

only rational one. Taking first his statistical returns one would be inclined to receive them as conclusive evidence of the superiority of his treatment, if we were not aware how fallacious statistics are frequently found to be. We know that certain men go into the great gambling houses, and with nothing to rely on but a run of luck break the bank as it is called; and it is possible that chance may affect the character of a man's patients, and therefore, indirectly, the results of his treatment. In addition, though I must admire the candour and the true professional zeal of Dr. Bennett, I cannot but doubt that he should include in his returns four fatal cases of pneumonic complications to which he refers, and which would make his cases 1·30; and that some weight should be attached to thirteen other cases which he says were found after death to have had pneumonia, which he, however, says was only secondary. The difficulty is to know whether other physicians would not have included in their returns cases of a similar character. I do not believe Dr. Bennett capable of straining a point to advance his favourite theory, but I cannot say the same of clinical clerks, who, as a class, are very well disposed to back up their master's views—*jurare in verba magistri*. I knew it to be the case when I was a student, and I suppose it occurs still. However the challenge thrown down to the profession is a fair one on the part of the distinguished professor, and would lead to beneficial results if medical statistics were not attended with difficulties such as I have already indicated.

I shall now briefly refer to the deductions he makes from pathology in support of his treatment. He states first, in contradiction to many other pathologists, that before the lymph, which is the product of pneumonia, can be absorbed into the blood it must first be converted into pus globules, which again are to be broken up, and disintegrated, and taken into the blood, and then to be eliminated by the kidneys or other emunctories. That these pus cells, to use his own words, must be regarded as living growths, and as such require an excess of blood, good nutrition, and exalted vital force to hurry on their development, and carry them successfully through the several stages of their growth. Now I shall not at all enter into the controversy amongst great microscopic observers, as to whether suppuration is an essential part of inflammation in every circumstance where exudation takes place, I shall merely remark that if it be, it is very different from what practical physicians call suppuration—that which causes the breaking down of tissue, and is productive of great constitutional as well as local changes, so different in kind and degree that giving them the same name is calculated to lead to great and serious errors in treatment. It is little matter to us how the process is conducted if we can prognosticate that the exudate is re-entering the blood without any destruction of tissue, and without constitutional disturbance, and that it is a salutary healthy change. The fact asserted

is, however, of little importance compared with the inference deduced from it—namely, that because pus cells are a living growth they require for their development excess of blood, &c. In order to see the correctness of the inference we must understand clearly all the terms of the proposition. Let any one answer me as to what is meant by an excess of blood? Would not this imply that there was some ascertainable quantity of blood which is to be taken as a standard, and above which standard the quantity of blood in one attacked with inflammation should be kept. We all believe that the quantity of blood in persons enjoying comparative health may he above or below what is most conducive to the perfect performance of their functions by all the organs of the body; and therefore we use the terms plethora and anemia to indicate the departure from this standard in opposite directions; from which it follows that an excess of blood may be detrimental to the performance of every vital function. Take as an instance the distressing state of many females in suppressed menstruation, unless where that suppression arises from pregnancy, when the increased quantity of blood finds a new channel without overloading the vessels of the mother; and when parturition has taken place the tendency to the formation of more blood than is necessary for the individual finds a vent at the mammæ. How often is headache relieved by epistaxis both in sickness and in ordinary health; and who is not acquainted with the fact that the drying up of habitual sores, or of eruptive diseases of the skin, or the cessation of a diarrhea in teething, will produce most dangerous pressure on some internal organ? We come then to the fact, that before the attack of pneumonia our patient might as well have had too much as too little blood to carry on the healthy functions of the body, and that so far as this circumstance is kept in view one patient may require stimulants where the other may require depletion. Now what is our experience? that whereas exudation, that is disease, takes place with a quick exalted action of the heart, absorption, which is recovery, takes place best when the normal condition of the pulse has returned. The continuance of the full quick pulse we look on as a continuance of the disease, even when our investigations do not enable us otherwise to discover its existence. There are many other vital functions going forward in the body at all times as well as the conversion of lymph into pus cells, and we do not consider it necessary to give stimulants to forward them. All the functions of life are never in so healthy a condition as immediately after recovery from a typhus fever, although the debility is then excessive, and the quantity of blood is at a minimum. The treatment here recommended is the same as that practised generally, with admitted benefit, in the lowest forms of typhus fever. From this one should infer that there is no essential difference between a fever and inflammation, which every one's experience contradicts. It was my lot very early in my practice to be called on by my senior consultant to bleed

an apparently strong man in fever. After taking about four ounces of blood he fainted, and I had to desist. I have often bled in pneumonia, and the patient invariably expressed a sense of relief—for the moment at all events. If the two diseases differ so much in their tolerance of depletion is it not likely they would differ equally in their tolerance of stimulants. Some years ago I was attacked with erysipelas. The disease we know to be debilitating, and I was not in strong health previously. My kind physicians ordered me to take some claret, but had reason to repent it on their next visit, and withdrew this mild stimulant. I am quite sure if I had been an hospital patient I should have taken twelve ounces at least in twenty-four hours, whatever might have been my feelings, provided this squared with the views of the attending physician. I have always considered that the best way to learn the treatment of disease is in consultation at the bed side of private patients. In hospitals men who are engaged in founding a new school of medicine have many temptations to prevent them from deviating from their written or expressed opinions in any particular case. Suppose for a minute that it is necessary an increased supply of blood should be kept up during inflammation, is wine the best for that purpose, or is not a wine-glass of milk capable of producing more blood, when digested, than the same quantity of wine. If we want stimulants we give wine, if nutriment, broths, or milk, or farinaceous food; certainly no one that I am acquainted with thinks of giving chops or beef-steaks during the existence of acute inflammation; still young practitioners will be inclined to think differently from reading the opinions of eminent writers. I have never been able to divest myself of the idea that in all acute diseases Providence has arranged for our safety in the best manner, and would not have so invariably removed the appetite in these diseases if a large supply of blood were necessary for obtaining a cure. Nature says you shall not eat. The doctor says you shall. Nature says you must have cold drinks. The doctor says you must have brandy. Which is to be obeyed? One more topic in connexion with this important subject I shall refer to. It is quite clear that bleeding and severe antiphlogistic treatment is not so much practised in this disease as in former times; and some assert that the character of the disease has become changed. Against this it is asked, is the whole constitution of men so changed that their diseases should partake of the alteration? Should not a wound received at the battle of the Alma undergo the same treatment as a similar wound received at Waterloo? But I think this question is wide of the point at issue. The character of many of the diseases from which we suffer depends on causes external to ourselves. For instance, the last epidemic of scarlatina that visited this city was different in many respects from the same disease occurring at other times. We know that erysipelas will affect hurts and wounds at one time which would not touch them at another; and every one

acquainted with disease on a large scale knows that pneumonia is sometimes absent from our hospitals for some years and then occurs with great frequency. Of course this must be owing to atmospheric causes, which if they are capable of producing the disease may produce it in variously modified forms. For my part, I rarely see cases of pneumonia in hospital till the stage of exudation has passed, and the restorative process has commenced. At this period no one would think of antiphlogistic treatment, which would be like destroying the furniture of a house with water after a fire had been extinguished. When I met cases in private practice in the early stage of the disease, I found the same advantage from bleeding or copious leeching as I did in my early days. I have also met cases both in and out of hospital which required stimulants from an early period, owing to the constitution and condition of the patient, irrespective of the state of his lungs. With reference to bleeding I hold very moderate opinions. If a physician chooses not to practise it he cannot be said to have done his patient harm; at most he has only withheld good from him. It is different with the giving of stimulants, which is a positive act for which he is responsible. In conclusion, the danger I apprehend to the profession from statistics is this—that assumed results would lead the young practitioner to think there is some royal road to the treatment of disease, and that he has only to make a correct diagnosis, and leave the nurse to administer the prescribed amount of stimulants and nutriment. Whereas it will be for ever the lot of the conscientious physician to tread his way cautiously in every case. To shut out from his mind all preconceived opinions, personal vanity, and self interest, and to treat each case on its own merits, and solely for the good of the patient.—*December* 13, 1865.

Albuminuria; Gall-stone; Suppression of Urine. By Dr. Finn.

John Heffernan, a butcher, aged fifty-nine, of intemperate habits, was admitted into the North Infirmary on the 16th November, 1865, suffering from abdominal pain, costiveness, and irritability of stomach, which symptoms had been in existence for a few days previouely. He had been an intern patient of the North Infirmary about two years since, labouring under obstruction of the bowels, on which occasion his convalescence was exceptionally tedious. During the past Autumn he had been treated with benefit in the South Infirmary, for anasarca, complicated with renal disease.

November 17th.—Costiveness and irritability of stomach persist; face and body generally jaundiced; occasional hiccup; pulse scarcely perceptible; urine very scanty, and deeply tinged with bile. On applying the hand over the abdomen, even with gentle pressure, great pain is experienced; but the pain presents its maximum intensity at a point corresponding with the free edge of the liver, where the gall-bladder is

situated. On the occasion of this visit an opinion was expressed that the jaundice was caused by a gall-stone, which occupied the ductus choledochus.

November 19th.—Costiveness slightly relieved by enemata, but the alvine dejections are destitute of bile. Complete suppression of urine. Death took place on the 20th instant.

Autopsy.—The viscera (both solid and hollow) were deeply stained with bile. The peritoneum was thickened and congested. The liver was enlarged and altered in form, the free edge being rounded off. A gall-stone of large size was found to occupy the commencement of the ductus choledochus, rendering the escape of the bile into the duodenum mechanically impossible. The form of the gall-stone was that of a drum; it measured two inches in circumference, and three-quarters of an inch in the vertical direction; several small gall-stones were also found, with a large quantity of cholesterine in the granular form. A section of the larger gall-stone presented a "nucleus," of considerable consistence, the portion intervening between it and the surface being quite granular and easily broken up: the superficial layer also possessed a moderate degree of consistence. The kidneys were hypertrophied and congested, well exemplifying that form of albuminuria which is characterized by morbid development of the structures in question.

Remarks.—The determination of bile to the kidneys, consequent on the occlusion of the ductus choledochus as already noticed, would appear to have overtasked the remedial activity of those organs, and thus concurrently with the hypertrophy of the renal structures to have induced suppression of urine, which was the more immediate cause of death.—*November* 22, 1865.

Epistaxis; Anasarca; Albuminuria; Hypertrophy of the Heart in the Absence of Valvular Disease. By Dr. Finn.

Michael Mahon, a labourer, aged thirty-eight (respecting whose antecedents little or nothing was ascertained), was admitted into the surgical wards of the North Infirmary, on the 25th Spetember, suffering from profuse epistaxis, which was only controlled by the plugging of the posterior nares, other means having failed to afford relief. When the epistaxis had subsided, anasarca immediately supervened, and contemporaneously with it effusion into all the cavities. When he was removed to the medical wards, his face was blanched to the last degree, and the difficulty of breathing amounted to orthopnea. His replies to questions were generally incoherent. Pulse quick, and excited. Tongue pale, quite in keeping with the pallor of the countenance. The secretion of urine was normal in quantity. Auscultation revealed nothing abnormal in the sounds of the heart. Death took place on the 14th November.

Autopsy.—Effusion to a large amount was found in the cavities of the pleura and pericardium. A good deal of superficial adipose deposit on the heart, near the base. The heart was remarkably pale, but firm in its consistence; it weighed twenty-one ounces and two drachms. The left ventricle was much hypertrophied; no valvular incompetency was found to exist. The kidneys were much diminished in size, and irregular on the surface.

Hypertrophy of the heart, in the absence of valvular disease, has been noticed by Dr. Bright as frequently co-existing with albuminuria.—*November* 22, 1865.

On the Constituents of Food, and their Relation to Muscular Work and Animal Heat. By F. C. DONDERS, M.D., Professor of Physiology and Ophthalmology in the University of Utrecht. Translated from the *Nederlandsch Archief voor Genees- en Natuurkunde*, 1e Deel, Utrecht, 1864, by WILLIAM DANIEL MOORE, M.D., Dub., M.R.I.A.; Honorary Fellow of the Swedish Society of Physicians, of the Norwegian Medical Society, and of the Royal Medical Society of Copenhagen; Examiner in Materia Medica and Medical Jurisprudence in the Queen's University in Ireland.

(*Concluded from page* 252.)

V. RELATION BETWEEN THE PRODUCTION OF HEAT AND OF MECHANICAL WORK.

As each form of energy may be changed into heat, it is evident that each element of food may, directly or indirectly, produce heat in the body. Of mechanical work the same cannot, *a priori*, be asserted. In this case, for the possibility of origin from all elements of food, the argument is wanting, that each form of energy should be capable of giving place to mechanical work. On the contrary, we know that heat can only partially pass over into mechanical work, and that even for this conditions are required which are not present in the animal body (compare p. 243). It was thus imaginable that certain substances, on oxidation, develop only heat in all tissues, and there was, so far, nothing absurd in the supposition that non-nitrogenous bodies could produce no mechanical work, that this depends simply and solely on metamorphosis of albuminous matters. But if this theory was not absurd, it was nevertheless extremely arbitrary, and also highly improbable. Consequently, it was by no means reasonable that it should be looked upon as proved. At most, it could be looked upon only as an hypothesis. Its untenability will have appeared so soon as it shall be shown that the chemical energy of the consumed albuminous matters is not equivalent to the mechanical work

performed. We propose to investigate this. To this end we shall endeavour to discover the relation between the quantities of mechanical work and of heat developed in the body, in order to be able to compare therewith those of the nutritive elements in the food used.

Traube supplies the following calculation :—[a]

A *working* horse produces 75 kilogrammeters per second, and consequently in 8 working hours 2,160,000 kilogrammeters=(2,160,000 : 430) 5025 calories.

A *non-working* horse consumes, according to Boussingault, in 24 hours, 2465·1 grammes of carbon and 24·4 grammes of hydrogen, affording on combustion to CO^2 and HO about (2465·1×9·6+24·4×44·462)=24506 calories. Supposing that the horse, in performing work, uses only so much more organic matter as is equivalent to the work done, Traube now finds the relation between the production of heat and of work=24506 : 5025, that is about=5 : 1, and for the working time, calculated at $\frac{1}{3}$ of the day= $\frac{24506}{3}$: 5025=8·2 : 5. Consequently, during the working time the mechanical work performed would amount to about $\frac{5}{8}$ of the work, represented by the heat produced.

This calculation suggests one or two remarks. In the first place, the assumption that, in work, greater consumption of matter would occur exclusively for this work, is very far from the truth. We know, in fact, that the production of heat in this case is also considerably increased, and experimentally double, nay five-fold excretion of carbonic acid has been observed. In the second place, the calculated horse-power is too great for an ordinary horse, to which the food calculated refers. We shall make a similar calculation from observations upon man, communicated, it is true, by Traube, but not employed for calculation, which will bring us to a completely different result.

Lavoisier[b] and Séguin found the consumption of oxygen by a man, not during the time of digestion, at 59° F., in the state of rest, for one hour, ... 26·66 litres,
During the performance of work, . . 63·477 „

For the increased consumption of . . 36·817 „

an amount of work was performed equivalent to the lifting of 7·343 kilogrammes to 799 metres, that is 5867 kilogrammeters=(5867 : 430) 13·583 calories. On the other hand, 36·817 litres=49·89 grammes of oxygen yield, on combustion with carbon, 179·616 ; on combustion with hydrogen even 214·905 calories. Hence it appears, that at most $\frac{1}{14}$ of

[a] Virchow's Archiv, B. xxi., page 391.

[b] Conf. Gavarret. De la chaleur produite par les êtres vivants, p. 330. Paris, 1855.

the greater consumption of oxygen was used for mechanical work; of all the oxygen consumed, not more than $\frac{1}{24}$.

In another case, Lavoisier and Séguin found the

consumption of oxygen in a man, during digestion at rest, again at 59° F. for 1 hour,	. .	37·689 litres,
During the performance of work,	. .	91·248 „
For the increased consumption of	. .	53·559 „

a work is here performed of 6201·8 kilogrammeters=14·423 calories, while for 53·559 litres of oxygen, burned in the body, 261·274 calories are obtained, so that of the increased consumption of oxygen, certainly not more than 14·423 : 261·274, that is $\frac{1}{18}$, and of the total consumption still less than $\frac{1}{30}$, is represented by the work performed. To these results, which would fully justify us in deriving all the mechanical work from nitrogenous combinations, we shall not attach too great importance, because we believe that the work performed in the investigations is estimated too low. But they form an admirable appendage to the calculations of Traube.

Coulomb calculates in the ascent of the Peak of Teneriffe, with De Borda, 204610 kilogrammeters per day of 8 hours, that is, 25576 per hour, or four times as much as was assumed by Lavoisier. For other work, such as the laying of paving-stones, Coulomb calculated much less, namely, 75240 kilogrammeters daily; Lamandé, 80635; Haughton, 124000 kilogrammeters, which, calculating the working time at 12 hours a day, amounts to little more than one and a half times the work found by Lavoisier. If we reckon 86000 kilogrammeters=200 calories, these are obtained by burning about 20 grammes of carbon of carbo-hydrates, while the daily consumption of carbon amounts to not less than 240. Consequently, even leaving out of view the hydrogen of the fats, &c., the work performed would be equivalent only to $\frac{1}{12}$ of the chemical energy used.—Under these circumstances, too, it was still possible to admit, that only albuminous matters produce this work.

In extraordinary exertion, under which head certainly the ascent of mountains is to be classed, the work performed is greater in reference to the heat produced. Helmholtz[a] supplies us with a calculation on this subject, based on the results obtained by Dr. E. Smith of London, who, with a compact gasometer, determined the quantity of air inspired under different circumstances (sleep and vigorous exercise not excepted), and, with a second equally portable apparatus, collected and weighed the carbonic acid expired during many hours. In the first place, Helmholtz deduces from the results obtained by Dulong and Despretz, from experiments on animals, how much the temperature of the human body ought to rise by

[a] Lectures on the Conservation of Energy, delivered at the Royal Institution, April 1864.

accumulation of heat, developed in one hour, in the condition of rest, and for this he finds 2°· 16 F. This heat is the equivalent of an ascent of the body to 1,760 feet. Now, by strenuous exertion, a practised climber of mountains can attain this height in the space of one hour. But in such exertion Smith finds that the expired carbonic acid is five times greater than in the state of rest, and as only one quantity serves for the ascent itself, four times the quantity remains over for the heat produced. Hence it follows, that the equivalent of mechanical work and heat, developed in active exercise, are to one another=1 : 4. In this nothing is brought into calculation for the loss of carbonic acid by the skin, which precisely on great exertion, in horses at least, is very considerable (Gerlach). For ordinary work, the proportion of 1 : 5 is certainly estimated highly enough. The result is therefore quite different from the 3 : 5 calculated by Traube. But this does not hinder us coming so far to the same conclusion, that we think the origin of mechanical work exclusively from nitrogenous matters inconsistent with this result.

We have here to refer to our demonstration, given in the preceding section. At the close of it (p. 252), we arrived at the result: that *in mechanical work, yielded externally, only a part of the elastic energy produced in the muscles is still found.* We remarked, that a second portion of the elastic energy—namely, what remained over in the subsequent relaxation of the muscle—must give place to heat. Evidently, however, with the elastic energy, the direct condition for mechanical work is already given. How far that will become for mechanical work depends on the loading, and is, in a certain point of view, accidental. Consumption of matter, in other words, chemical energy, is in any case no longer necessary thereto. We have, therefore, for comparison with the nutritive principles used, to know the relation between the elastic energy and the heat; and if we assume that the half of the force occurs not as mechanical work, the proportion alters from 1 : 5 to 2 : 4. If we consider, further, what numerous muscles come into action in each movement, without contributing directly to that movement, how much elastic energy in respiration, how much mechanical work in the circulation of the blood, and, moreover, in other functions, are changed into heat, we may for this also set down for $\frac{1}{6}$ part transition of elastic energy and muscular work into heat, and we then obtain for the original relation of elastic energy to all forms of work, that of 3 : 3; in other words, about the half of the chemical energy of our food is changed, in the muscles, into elastic energy.

Now, in ordinary feeding, we may calculate for man:—500 grammes of carbo-hydrates, to 100 grammes of fat and 100 grammes of albuminous matter. In general, these last will represent not more than $\frac{1}{3}$ of the chemical energy. If, therefore, all albuminous matters were employed for muscular work, which is by no means the case, they would be

insufficient completely to explain this. It is, therefore, certain that, besides the albuminous matters, there are constituents in our food from the use of which elastic energy is produced in the muscles.

VI. NITROGENOUS PRODUCTS OF METAMORPHOSIS OF TISSUE.

In Traube's theory we see the most radical reaction upon the doctrine maintained by Liebig. If, according to the latter, mechanical work could be produced only from albuminous matters, Traube inverts the proposition, and asserts that in such work exclusively non-nitrogenous substances are used, upon the oxidation of which the nitrogenous are said to have only an indirect influence. In presence of this assertion, our task is quite different. If we had, above, to prove that not all mechanical work takes its origin from nitrogenous matters, we must now examine whether any direct share therein belongs to them. The solution appears to be attainable by determining the excreted matters. By comparative investigation of these, both qualitatively and quantitatively, in the conditions of rest and of work, it may certainly be found, what matters in the latter case are used in greater quantity. Now it is known that in work much more carbonic acid is excreted. We may further assume, that in proportion herewith the formation of water in the body will be increased. But, on the contrary, according to the investigations of Voit, the eliminated nitrogenous compounds are not essentially increased. Great weight is attached to this result. It has led Traube to apply his fermentation theory to the metamorphosis of matter in the living muscle. Now, have these investigations actually proved that in muscular work the metamorphosis of nitrogenous bodies is not increased?

As is well known Bischoff and Voit assume, that all the nitrogen disappears from the body by the kidneys and the intestinal canal—in the dog almost exclusively in the urea of the urine. In the first place, we have to inquire how far this proposition has been proved.

The question, whether nitrogen is excreted by perspiration, always presented great difficulty. If, on analysis, the nitrogen of the expired air was found to be greater than that of the inspired, it was very evident that this did not prove the fact of an absolute increase: the diminished volume of the air might equally account for it. An attempt was now first made to answer the question indirectly. If the absolute quantity of nitrogen, both in the food and drink, and in the feces and urine, were determined, the difference ascertained must represent the quantity of nitrogen which has escaped in the perspiration. Boussingault found this to be in the cow not less than $\frac{1}{6}$; in the horse more than $\frac{1}{7}$; in the turtle dove, $\frac{1}{3}$ of the nitrogen of the food. Barral, in man, found it to be about one-half; Sacc, in chickens, still more than one-half. On the

[a] Art. Harn. Wagner's Handwörterbuch f. Physiologie.

other hand, C. G. Lehmann[a] had, under the use of highly nitrogenous diet, recovered nearly all the nitrogen of the food in the urea, while Jörgensen, in experiments continued for a month upon a sheep, found in the feces and urine 93 per cent of the nitrogen. Lastly, Bidder and Schmidt had, in some experiments upon dogs and cats, recovered nearly all the nitrogen of the food in the urea. The question now attracted the attention of Bischoff. He applied to Liebig for a volumetric method of determining the urea of the urine, and Liebig gave him, in the solution of nitrate of mercury, a fluid which answered his requirements beyond expectation. According to Voit, in fact, not only the urea is precipitated by this fluid, but also the other nitrogenous constituents of the urine, and indeed almost in the inverse ratio of their nitrogenous element; so that what the boldest imagination had not dared to expect was ascertained, the amount of nitrogen in the urine, no matter in what combination it might exist.

Meanwhile, Bischoff found at first not nearly all the nitrogen of the food in the urine; and it was therefore rather strange that he, on this ground, sought the measure of the metamorphosis of tissue in the quantity of urea excreted by the urine. The investigations of Voit, which soon after followed, appeared rather to prove this measure to be acceptable. But even in them, too, the nitrogen of the urine and feces, after correction for the altered weight of the body, always remained below that of the meat used, notwithstanding that only the minimum of the nitrogen found therein was taken into account,[a] and I therefore do not hesitate to assert, that neither did these experiments exclude the possibility, that about 10 per cent. of the urea had escaped in other ways. Haughton now finds, moreover, in man, in contrast to Barral, nearly all the nitrogen of the food in the urea and feces. Likewise Ranke, in a series of investigations which bear the best stamp; so that Barral's result is to be considered as refuted. In horned cattle, too, Henneberg found the deficit in general less than that of Boussingault; where the supply of nitrogen was small, the deficit was comparatively great—thus also there was a deficit, under circumstances in which an addition of flesh in the animal was not to be assumed. In his subsequent investigations Voit obtained, not only in four dogs anew nearly all the nitrogen of the food from the urea, but also in a pigeon which he (in order to test Boussingault's results) had fed for 124 days

[a] Voit found in fresh meat, as maximum, 3·73 per cent., as minimum, 3·41, and as average, 3·59 per cent. of nitrogen, and brought into calculation only the minimum; that is, 5 per cent less than the average and 9 less than the maximum. In the calculation, too, of all subsequent experiments, he has kept to the minimum. The reason he assigns for this is, that the meat analysed was much more accurately freed from adipose tissue than that intended for food: it is, indeed, not difficult to free meat from adipose tissue to less than 1 per cent., and I believe that Voit will certainly have done this.

with peas, containing a known amount of nitrogen, the quantity of that element in the excreta being constantly determined; no deficit worth mentioning (1·1 gramme) was, after correction for a slight increase of weight, met with. Finally, an experiment upon a bull, detailed, at length, by Grouven[a], is in favour of the elimination of nearly all the nitrogen in the feces and urine. This writer, now assumes in all further investigations and calculations, that no nitrogen escapes with the perspiration, though not, as he expressly says, because he is as fully convinced of the impossibility thereof as Bischoff and Voit, but—because otherwise the results of his investigations would not be of much use. Grouven has also convinced himself that only traces of ammonia escape, which are not taken into account: indeed, the debates upon this subject are at an end.[b]

But is it now really certain that all the nitrogen is excreted by the kidneys and the intestinal canal? that not a trace of nitrogen disappears through the lungs or skin?

To the results of indirect experimentation above detailed those of direct investigation are strictly opposed. I allude to those of the investigations of Regnault and Reiset, continued for years by Reiset, and engaged in on a smaller scale also by Marchand. Regnault and Reiset caused animals to breathe in a closed space for about 24 hours, while, by a peculiar mechanism, the carbonic acid formed was constantly absorbed, and was replaced by an equal volume of oxygen. Where they found, at the end of the experiment, the absolute quantity of nitrogen in the enclosed space increased, they inferred that nitrogen was excreted by perspiration. Voit made it appear, as if Regnault and Reiset found the nitrogen at one time increased, at another diminished, without any assignable cause, or any regularity. In this he is deceived. The experiments show that, in warm-blooded animals, the quantity of expired nitrogen is usually less than $\frac{1}{100}$, often amounts to only $\frac{1}{200}$, and never exceeds $\frac{1}{30}$ of the oxygen consumed. But only under particular circumstances, namely, after the withholding of food, almost without exception in birds, sometimes also in mammalia (in marmots during hybernation), was the nitrogen, in place of being increased, found to be diminished. Reiset continued these investigations for years, and his results communicated last year to the Institute, of experiments upon sheep, calves, and wild boars, are similar to the former conclusions. Marchand, too, found for 100 volumes of carbonic acid 0·94 volumes of nitrogen expired.

All these investigations were blotted out by Voit with a stroke of his pen. We may regret with him that Regnault and Reiset appear to have

[a] Physiologisch-chemische Fütterungs-Versuche. Berlin, 1864, p. 121.

[b] Conf. Kühne and Strauch. Centralblatt f. d. mediz. Wissenschaften, 1864, Nos. 36, 37.

neglected to control the closing of their apparatus. But has he the right, therefore, to set aside these results without having repeated the experiments in a controlled and correct apparatus, with a negative result? With such experiments he would much better have silenced his adversaries, than with a philippic on their obstinacy. It is evident that in the controversy facts are arrayed against facts. We shall not defend the results obtained by earlier investigators against those that are more recent. We acknowledge that in the force of some results of indirect experimentations there is little to object to. According to the experiments so long continued by Voit upon one and the same pigeon, we must believe that a pigeon can live without losing nitrogen, otherwise than by the feces and urine. But all the other experiments, even those of the more modern observers—leaving out of consideration the frequent deficit of nitrogen—admit the possibility, partly on account of the short duration of the experiments and the impossibility of bringing into account with perfect accuracy the change of bodily weight—partly on account of the uncertain amount (probably calculated too low) of nitrogen in the meat used—partly (in horned cattle) on account of the very variable contents of the intestine (Grouven)—that some nitrogen, as such, disappears through the perspiration. These experiments can, consequently, not refute the existence of this excretion; and with our eye upon the results of the direct experimentation, we are not at liberty flatly to deny it.

VII. INFLUENCE OF MUSCULAR WORK UPON THE PRODUCTS OF METAMORPHOSIS OF MATTER. OBSERVATIONS OF VERLOREN AND OF VOIT.

In the first place, I may here communicate the observations of Dr. M. C. Verloren. They were made and communicated in our circle, "amicitia naturæ interpres," at least two years before the investigations of Voit were known, and before Traube established thereon his theory. On my requesting that he would be good enough to give me, in writing, the leading points of his statement, I had the satisfaction to receive the following answer, which will certainly be interesting to my readers:—

"The observations upon insects, which had already led me to the conviction that muscular work is attended with no considerable metamorphosis of albuminous matter, consist in the following:—

"1°. Many insects use, during a period in which very little muscular work is performed, food containing chiefly albuminous matter; on the contrary, at a time when the muscular work is very considerable, they live exclusively, or almost exclusively, on food free from nitrogen.

"2°. During the first period a very great quantity of urine is excreted, in proportion to the respiration, and the respiration also is absolutely little; during the second period, on the contrary, the urinary secretion is

slight in proportion to the respiration, and the respiration also is very considerable.

"Of the first proposition I formerly quoted bees as an example, and they have since been indicated also by others. But as I do not know whether this argument has been brought forward in its full force I shall say something on the subject.

"Bees have three sorts of cells: 1°, for working bees; 2°, for male bees; 3°, for queens. The first are by far the most numerous, the smallest, and are of the same form and capacity as those in which the honey is collected and kept; the second are somewhat larger; and the third are still larger and of a totally different shape. In the bottom of each of these cells the queen lays an egg. After some days a worm-like larva emerges from the latter, lying on the bottom of the cell. The working bees then daily bring these larvæ food, which they pour out into the cells. This the larvæ appropriate, become larger, and at last fill nearly the whole cell; they are then full-grown, and spin a dense web, closing the openings of the cells from above. They then no longer receive any food, but undergo this change, whereby a completely new being is formed out of the accumulated food, with totally different organs and tissues. When the bee is fully formed, it gnaws away the spun covering, and comes out. It now enters upon its active period of life, in which, during a considerable time, it performs enormous muscular labour in its numerous and very rapid flights, in its great industry and vivacity; bees are, in fact, the most restless and most lively of insects. In the larva state, on the contrary, their muscular work is as slight as possible, being confined to swallowing the food, turning occasionally in the cell, and spinning the closing material of the same.

"Now, of what does the food of bees consist? Of the so-called *bee-bread*, that is pollen from the flowers, with a little honey; and, 2ndly, of honey. The first, containing a large quantity of albuminous matters is exclusively the food of the larvæ; the second of the full-grown bees, which use very great quantities thereof, as is evident from the considerable amount of it which they accumulate, to serve as a store during the Winter, and when from other circumstances, they cannot go to fetch it. In the Winter there are no larvæ.

"I have already referred to the butterflies. The caterpillars, it is true, develop more muscular work than the larvæ of bees; they must themselves, crawling, seek their food, must spin more, or crawl more, to seek for a suitable place for transformation to the chrysalis state. Still this is only inconsiderable when compared with the action of the restless flying butterflies. Now, the caterpillar feeds on leaves containing albuminous matter; but the flying butterfly seeks only honey from the flowers; their mouths are even not adapted for the reception of any other food.

"I now pass over to the second point, from which alone the first derives its full force, and to which my investigations have more decided reference.

"During the larva condition there has been formed from the food consumed, in addition to the organs of the caterpillar or larva, a thick layer of adipose tissue, consisting of albuminous matter and fat, situated beneath the skin with its muscles, and surrounding the intestines. In the chrysalis state this adipose tissue again becomes fluid, and is taken up into the blood, as are likewise the existing organs of the caterpillar, and out of these materials totally new organs and tissues are formed, producing an entirely new being, the perfect insect. During this transformation no more food is taken in, but what is already present in the animal is quite unmixed and ample. Neither are any excrementitious matter formed and voided; the respiration alone continues, so that the chrysalis constantly decreases in weight. In this metamorphosis of the organs, and in the course of the other vital functions of the insect, urine is, at the same time, in connexion with the respiration, secreted; this, however, is not evacuated, but collects in a great vesicular process, which arises at the rectum, in the formation of the organs of the perfect insect. Immediately upon, and shortly after leaving the chrysalis envelope, the perfect insect expels the whole quantity of urine formed and collected during its chrysalis existence.

"I must also make mention of the prolongation beyond a year of the chrysalis state, a phenomenon which takes place in some insects. Most insects are in their different states bound to definite periods of the year. Now, it happens in some insects, that when the time for the chrysales to emerge is past, some have not come out; these, then, lie over for another year, in order then to emerge at the same time as those which did not pass the year. In *Sphinx Ligustri* this often takes place. It is to that insect that the following results refer. It remains about 300 days in the chrysalis state, so that those which pass the year, continue in it for 665 days. The investigations were made on a certain number of insects, varying for the different investigations, but separate for each individual. The result here given is the average of the several observations.

"The following chrysales have thus, during the chrysalis state, given, in urinary secretion and respiratory loss, calculated in per centage of the weight of the chrysales on the 66th day before the emergence of the butterfly, as follows :—

		Urinary Secretion.
Annual,	♀ ♀	45·89 per cent.
„	♀ ♀	31·76 „
Biennial,	♀ ♀	35·64 „
Further urinary secretion for the additional year,		3·88 „

	Respiratory loss.	
Annual,	10·71	per cent.
„	10·43	„
Biennial,	10·40	„ respiratory loss in 300 days.
Further urinary secretion for the additional year	7.74	„ respiratory loss in the additional year,

"The relation of the urinary secretion to respiratory loss in females for one year, and for the extra year in the biennial females, is therefore:—

For 100 urinary secretion, 32·75 respiratory loss in those of one year.

„ „ „ „ 199·48 „ „ for the extra year.

"The butterflies have yielded in urinary secretion and respiration, per hour, in milligrammes:—

	♂	♀
Urinary secretion,	0·326	0·425
Respiration,	1·253	1·252

"For the butterflies I cannot easily state the quantities in per centage upon the bodily weight, but for the relation between the urinary secretion and respiration, which is here principally in question, this is not necessary. We see that this is about as follows:—

Chrysales.				Butterflies.	
Annual, ♂	Annual, ♀	Biennial, ♀ Extra year.		♀	♂
4	3	½		⅓	¼ Urinary secretion to respiration,

so that in the male chrysalis fifteen times more urine is excreted than in the male butterfly.

"The greater urinary secretion in the male chrysales than in the female, is constant. Now, the chrysalis performs scarcely any muscular work; the butterfly an enormous amount."

"When we take into account that the ♂ chrysalis excretes nearly half its weight in urine, when we add thereto what is lost in respiration in the chrysalis envelope and in the exhalation of the butterfly on emerging, which is very considerable; when we, furthermore, reflect that the butterfly consists only in small part of muscles, and that it takes in *no more albumen whatever*; it cannot be otherwise than that for muscular work but very little albumen is metamorphosed."

"As the butterfly does not weigh the half of the chrysalis, and nearly the half of the chrysalis passes into the urinary secretion, the whole butterfly would necessarily be wasted away if it would consume as much albumen as the chrysalis. But the muscles and the albumen still present, which might be used for muscular work, will not amount to a fourth part of the butterfly. If, therefore, the butterfly entirely consumed its muscles, it would have used only a fourth of the amount of

albumen used by the chrysalis. But muscles, which are no more cannot perform any function, so that only a very small portion of the muscle can be consumed if it is still to remain in a serviceable state. I will assume a tenth, in which case the butterfly has at its disposal only a fortieth part of the quantity of albumen consumed by the chrysalis. And yet the butterfly performs muscular work, and the chrysalis almost none.

"It seems to me that it is as clear as possible that the albuminous matters exclusively, or at least almost exclusively, are metamorphosed in the formation of the tissues and organs, and that the non-nitrogenous nutritive matters, by their disintegration, produce work.

"I will yet remark, that the observations on the butterflies deserve the least confidence, but that in more accurate essays they would tell still more to my advantage, that probably the respiration will prove to be still greater, and the urinary secretion as much less. Also that the first three respiration numbers in the chrysales must really be somewhat higher, but too little so to have any influence upon the results."

In heartily thanking my friend Verloren for the communication of his investigations, which are as important as they are accurate, he will not take it ill of me if I bring under the eye of our readers my reflections, which are well known to him, against the inference he has drawn from them. That the larvæ, in their development, that is, in the building up of albumen-containing organs, take in albuminous matters in their food, is not to be considered strange. Out of albuminous matters the young bird in the egg is also principally developed; and the embryo of the mammal, which, except the movements of the heart, certainly performs very little work, needs for its development also chiefly albuminous matters. When the larva is full-grown, a period of quite peculiar metamorphosis ensues. That then, while from the old a new being is formed, many albuminous matters should be metamorphosed, cannot surprise us. The phenomenon simply proves that metamorphosis of albuminous matter exists without any connexion with movement, and on this point there is no room for doubt. From the experiments of C. G. Lehmann and Hasse we have already learned, that feeding with highly albuminous matters, independently of muscular work, considerably increases the excretion of urea, and in the excreted urea we can consequently find no measure of work performed. Further, it appears inadmissible to compare the consumption of matter of the perfect insect with that of the chrysalis state, with which such peculiar nutritive changes are bound up. That consumption of matter must be considered by itself; and then the question is only, how much muscular work the insects perform, and how much nitrogenous and non-nitrogenous matter is at the same time metamorphosed. And as to this work: by the remark, that the animals are quick and lively, and perform long

journeys, it is not measured, as little as all nitrogenous products of the metamorphosis of matter are determined by the excreted uric acid. Lastly, I am somewhat sceptical as to the composition of the food taken in. Does the butterfly suck so exclusively non-nitrogenous honey out of the flowers? Precisely in Winter, when the bees do not stir, this non-nitrogenous honey appears to be sufficient. Undoubtedly, the facts collected by Verloren are important; they show, that in insects much albuminous matter is used, and much uric acid formed, without the intervention of movement. They shed light upon the metamorphosis of matter connected with the several stages of insect life. But to the question, how much nitrogenous matter is consumed in reference to the equivalent mechanical work they give no satisfactory answer.

We now come to the investigations of Voit. They are confined to the determination (by Liebig's volumetric fluid) of the amount of urea contained in the urine of a dog both during abstinence and during regulated feeding with meat, in both cases with and without the performance of mechanical work.

The work which the dog had to perform consisted in running in a tread-mill. Voit calculates this (indeed somewhat liberally) at 150,000 kilogrammeters daily. The observations extend over four series; 1°, one of five days, during the withholding of food; on the second and fourth day with work, 16·6 grammes of urea were excreted; on the three other days, in a state of rest, the amount was 14·3 grammes. 2°, one of nine days, food being likewise withheld, on the fourth, fifth, and sixth days, with work, 12·33 grammes were excreted; on the other days, the average amount was 10·88 grammes. 3° and 4°, two series, on each of three days' work, followed by three days of rest, begun after an equilibrium of ingesta and egesta had been attained by regular feeding with meat; on the days of rest the excretion amounted on an average to from 109 to110 grammes; on those of work to 114, when the labour was performed before eating (3rd series); to 117, when it was performed after eating (4th series).

These are briefly the facts. They lead to the conclusion that during work the metamorphosis of nitrogenous matter in the muscles is not increased. Theoretical considerations connected therewith, to which Voit himself will certainly no longer attach much value, we leave out of the question. It is with the direct conclusion we have to do, and against this we have some difficulties to bring forward.

In the first place we observe, that on the days of work actually somewhat more urea was found, which Voit, without sufficient proof, thinks is to be ascribed to the increased excretion of urine. A second difficulty relates to the method followed. Does the volumetric fluid really show the amount of nitrogen present? Besides urea there are several nitrogenous matters in the urine; and Voit thinks he has satisfied

himself that the amount of nitrogen in these also is approximatively shown by the volumetric fluid—so soon as one or other of these matters, under the extraordinary circumstances of violent muscular work, is disproportionately increased, we can certainly no longer rely upon the method. Now, to deny, *à priori*, the possibility of such an increase, because even in the state of rest also a number of muscles are active,[a] appears to me presumptuous; and, looking to the results of experiments obtained on other animals with respect to hippuric acid, inadmissible. I, therefore, with Meissner, think it desirable, in such experiments, to determine the amount of nitrogen directly. Our third difficulty is, that Voit feels himself justified in neglecting the loss of urea by the skin, and in denying the loss of nitrogen by perspiration. How great the amount of urea of the sweat in work may be, we know not. But in a dog, which wearies itself in the highest degree by work, it may come into consideration. Remarkably enough, Speck found in a man, that the urea of the urine was not increased only when the work was accompanied by copious perspiration. And as to the escape of nitrogen in the perspiration, we have in the preceding section shown that the results of the direct investigation are not satisfactorily refuted by those of the mediate. Now, if there is such a loss in the state of rest, there is every reason to assume that in muscular work it will be considerably increased. Even for these reasons we think that the conclusion, that in muscular work the metamorphosis of nitrogenous matter is not increased, is liable to doubt. But there is more.

What is the origin of the urea, determined by Voit. Is it wholly in the muscles? Certainly not. For the greatest part, at least? This, too, is improbable. In the nutritive fluid of muscles we find creatin, creatinin, and inosinic acid, no urea; and how far the substances mentioned are changed into urea, has not been ascertained. In every case, too, creatin and creatinin occur in the urine; and no one has shown that these should not be met with in greater quantity in muscular work. Evidently, therefore, the metamorphosis of nitrogenous matter in the muscles might be considerably increased without much of it being discovered by the volumetric method applied to the urine. It should not be forgotten that, independently of muscular work, much urea is produced—the more so, in proportion as more albuminous matter is supplied to the body; and in order to establish the increased consumption in the muscles, it is therefore certainly not enough to compare the ascertained increase of nitrogenous products of the metamorphosis of matter with the sum total of these products. It should further be borne in mind, that a greater metamorphosis during work, is followed by a slighter change during rest (Speck), and that this subsequent diminution

[a] Conf. Voit, in Liebig's Annalen. 1863. II. Supplement-Band, p. 376.

might conceal the greater consumption of nitrogenous matter during work.

Lastly, we have to refer to a singular result, to which Voit's theory would lead us. It is proved that the production of carbonic acid increases very considerably in muscular work. In a good walk, in which from two to three English miles (three miles are nearly an hour's walk) are performed in the hour, Smith found the quantity of carbonic acid expired from $1\frac{1}{3}$ to $2\frac{2}{3}$ times greater than in the state of rest; in ascending a mountain it was five times greater; and when seven miles were accomplished in the hour the quantity of carbonic acid was multiplied seven-fold. Now, supposing that a dog, on pure meat diet, daily performs work, by what other matters can the increased quantity of carbonic acid be permanently supplied if not by the meat consumed? And if the nitrogen were not at the same time excreted, and indeed in the same proportion, should we not finally have the prodigy of a dog existing on pure nitrogen? This is what is called an *argumentum ex absurdo*. It may at least prove that experiments such as those of Voit, in order to lead to certain results, must be continued much longer on the same animal.

VIII. METAMORPHOSIS OF MATTER IN THE MUSCLES.

Not only the excreted products of the metamorphosis of matter, of which we have been speaking, but also the products in the muscles themselves may throw light upon the matters consumed in work. That the consumption during work is more active than in the state of rest is easily proved. In the first place, it is already included in the mechanical work produced together with increased warmth in the muscles themselves, which can have no other origin than than the chemical energy of the matters consumed therein. But, moreover, it appears directly in the examination of the outflowing blood. Already Bernard had observed, that in contraction of the muscles this blood is particularly dark; and Ludwig and Sczelkow[a] once found only 1·3 per cent. of oxygen remaining therein—only about the third part of the average quantity in the state of rest. Remarkably enough, the volume of carbonic acid obtained was sometimes greater than that of the oxygen which had disappeared. The question now is, what matters were here used? If the proportion of oxygen which has disappeared, and of the carbonic acid obtained, just mentioned, be established, we should be readily inclined to place the oxidation of carbo-hydrates quite in the foreground. But even then the hypothesis would still be required that a part also of the oxygen of the carbo-hydrates should have served in the production of carbonic acid, with the simultaneous development of a compound abounding in hydrogen.

[a] Sitzungsberichte der Wiener Academie. 1862, B. xlv., p. 171.

But we should hesitate to take refuge in such a supposition, although Pettenkofer and Voit, as is well known, in the calculation of excreted carbonic acid and absorbed oxygen (the latter, however, having been very indirectly determined) in dogs, fed partly with sugar or starch, arrived at the same unexpected result. But we certainly admit that sugar is consumed in the muscles. A small quantity of sugar actually exists in the muscles (Meissner), and sugar is also constantly conveyed with the blood. The same is true of the fats. The muscular tissue itself contains no inconsiderable amount thereof, and the blood incessantly supplies fats. It is true that it is conceivable that both fats and carbo-hydrates may be formed in the muscles themselves, from albuminous matters (Meissner assumes this of the sugar found to exist there); but with the great quantities of mechanical work and heat, which in fatigue of the whole body, may be supplied by some few muscles, without alternation with rest, the necessity of the supply of matters from the blood, during working, is to be admitted. In the never-resting heart it can even not be otherwise.

Neither has the examination of the nutritive fluid of the muscles been neglected. Helmholtz began with a comparative determination of the extractive matters, and found after contraction those soluble in alcohol increased, those soluble in water diminished. The lactic acid of the muscles has become important, especially from the investigations of du Bois-Reymond, who proved that it is formed in greater quantity in contraction, so that even during life an acid reaction may predominate just as occurs in all muscles after death, when the nascent lactic acid is no longer neutralised and carried away by the blood. From what matters the lactic acid is formed in the muscle is not shown. Perhaps the fact that in dogs the acid reaction during life occurs with great difficulty, while in rabbits it takes place very readily, may be brought into connexion with the difference of the alimentary principles used.—It is further known that here, too, nitrogenous products of metamorphosis of matter are developed, among which creatin and creatinin occupy a prominent place. Bearing in view Traube's theory, it was certainly an important question, whether these are produced in greater quantity during the action of the muscles. The investigation carried on by Sarakov, under the guidance of Kühne, has now answered this question in the affirmative; acid muscles are found to be richer in creatinin than alkaline muscles are; and after tetanisation the amount of creatin and creatinin was sometimes more than doubled.[a] Even many years ago, Liebig and Gregory found[b] in the flesh of a fox, which had been for 200 days well fed in his kennel, and had become fat, not the tenth part of the creatin met with in the flesh of one shot in the chase.

[a] Virchow's Archiv, xxviii, p. 544.

[b] Ann. der Chemie und Pharmacie, B. 74, p. 100.

The inference to be deduced from all this is evident: nitrogenous matters, too, are, during the action of muscles, metamorphosed in increased quantity.

In the metamorphosis of matters lies the cause of the fatigue of the muscle. The principle agent herein is certainly the accumulation of the products of the metamorphosis of matter in the organ. We have above (p. 248) already shown, that tonic tension of a muscle, without any external work, very rapidly produces fatigue, in consequence of the impeded removal of the products of metamorphosis of matter. The investigations of Ranke[a] fully corroborate this. They show that the removal of these matters is sufficient to remove fatigue, while injection of these matters (the extract of fatigued muscles, or even of lactic acid alone) suffices forthwith to elicit the phenomena peculiar to weariness, which then again give way on subsequent neutralization. The significance of the acid reaction for fatigue is confirmed also by the fact that in the never-wearied heart this reaction is absent during the whole of life—without doubt in consequence of conditions favourable to removal. But, on the other hand, we must not overlook the truth, that the supply of matters to the muscle is also a condition of its action. The fact that fatigue after work is more slowly recovered from than that after simple tonic tension, though also continued to total inability, may very well be connected with the need of new matters. In fact, it seems not possible that the removal of the existing products should require so much time as that during which the fatigue lasts. It is certain that the muscle has by continued action become in many respects another organ, in which the metamorphosis also is not yet equal to that occurring in a state of rest. This change of the organ, the altered composition especially of the nutrient fluid, must have a many-sided influence on the interchange of its constituents with those of the blood. That on the occurrence of the acid reaction the supply of albuminous matters will be limited, is in an ingenious manner brought by Heynsius into connexion with the periodicity of many vital phenomena.

IX. APPLICATION OF THE THEORY.

For practical application the theory is of little importance. We must be modest, and acknowledge that respecting the influence of muscular work on the metamorphosis of albuminous matters, we do not know much more than might in all strictness have been assumed since the investigations of C. G. Lehmann. By these it had already been shown, that with the use of a great quantity of albuminous matters the urea of the urine is considerably increased, and that in muscular work

[a] Archiv f. Anatomie, &c. By Reichert and du Bois-Reymond. Jahrg., 1863, p. 422.

this increase (from 32 to 36 and 37·4 grammes) is comparatively slight. Others came to the same conclusion: in the first place, Draper, who, on this subject, even thought it necessary to resort to the excretion of nitrogen in other ways; moreover, L. Lehmann, who met with increase only in a boy, and in a slight degree in women, not in men. In Speck's experiments the increase was somewhat more considerable. But in all we arrive at the result that, if only the ascertained excretion of urea be taken into account, in comparison with the augmented production of carbonic acid, the metamorphosis of non-nitrogenous matters in muscular work increases in a much greater degree than that of the nitrogenous. We readily admit this. We believe even that, as all excretion of nitrogen might have been taken into account, at least in experiments extending over only a few days, the augmented consumption would still have been found to be greater in the non-nitrogenous matters: for these few days of work fat of the body may be consumed.

But all these results bring us not a step further. They show only that non-nitrogenous and nitrogenous matters are both necessary for muscular work, as they are for life in general.

Two extremes were maintained, which appeared to be in a position to neutralise each other.

A few years ago Bischoff and Voit still held to Liebig's theory:—"It is and must continue true in all ages, that only these nitrogenous substances are the creators of force, that is, that they alone, by their metamorphosis in the animal body, give rise to effects of power, to motor phenomena. And in like manner it will remain incontrovertible, that the fat and the so-called carbo-hydrates, by their metamorphosis, produce only heat and no motor effects." And soon after Traube comes forward and asserts, with equal over-confidence:—"The organized part of the muscle is not destroyed in its work." "The metamorphosis of matter in the organized part of the muscle is neither connected with its action, nor is it increased by the latter." "In muscular action albuminous bodies in general are not destroyed." I would refer especially to the positiveness of these last words. It is remarkable that Traube erects his diametrically-opposite theory upon the results obtained by the experiments of Bischoff and Voit. The latter have now indeed given up their exclusiveness, and we soon find Traube also vacillating:[a]—"My theory does not, indeed, require," he says in a subdued tone, "that exclusively non-nitrogenous substances should he employed in the maintenance of muscular action, but it requires that the excretion of urea should not increase in proportion to the muscular activity." To such a modest postulate we can have no objection. To it not a single fact is opposed. It answers any mode of feeding; for, as the metamorphosis in the body

[a] Virchow's Archiv, B. xxiii., p. 196.

never increases in so great proportion as the muscular work, even in continued exclusively animal diet, all the nitrogenous products cannot increase at all in proportion to the work performed. But I repeat, on practical ground this brings our knowledge not one step further. It does not teach us with what relation between nitrogenous and non-nitrogenous alimentary principles an animal or a man is best adapted for muscular work, and this was the problem we had to solve.

Of one thing we may be convinced. What we give in the food, and what maintains the body in a comparatively healthy state, will also be used in muscular work. If we give more albumen, the metamorphosis of albumen increases: if we give more fats and carbo-hydrates, the consumption of these in the muscles must also increase. In the greatest differences in diet, in fact, so much work is performed, that in the one case the nitrogenous, in the other the non-nitrogenous matters of the food taken in are undoubtedly insufficient to explain all the work, and they must consequently in muscular work be capable of in part replacing each other. The question is not at present how far the nitrogenous matters first yield non-nitrogenous, which are then further oxidised; the fact is that the nitrogenous are consumed, and, as the increased amount of creatin in muscular work proves, in the muscle itself. It is, indeed, a well-ascertained fact, that under the most varied proportions of the alimentary principles in the food consumed, some men accomplish violent labour, others indulge in a life of ease. In the former as well as in the latter, these proportions admit of a wide range. The system accommodates itself. We were already aware, and fresh experiments of Voit have more precisely proved and explained, that the body cuts its coat according to its cloth. If much food be given it, it uses much; with a scanty supply it observes a wise frugality. This holds good of food in general, and it is applicable also to each of the alimentary principles. If to a regularly-fed dog we diminish the supply of albuminous matter, the supply is at first exceeded by the consumption; but the diminution of albuminous matters in the body, which is the consequence hereof, gradually lessens also the metamorphosis, and thus the equilibrium between supply and waste is quickly restored. On the other hand, with a more abundant supply of albuminous substance, more of the latter remains in the body, and the consequently increased metamorphosis in this case also restores the equilibrium between supply and consumption. These results, obtained by Voit in the dog, were confirmed by Ranke in the human subject. He obtained, so far as the excretion by urine and feces could show it, an equilibrium in two series of experiments, once with a proportion of nitrogen to carbon = 1 : 11, and again with the proportion of 1 : 15.

Now, what proportion is the most suitable to man for the discharge of his functions in general, as well as for the production of muscular work.

With this question we turn to direct experience. From it alone an answer is to be expected:—That in different animals, or even in different men, according to individual peculiarity, another proportion will be required, is evident, *a priori*. We are therefore to look for no definite conclusion, where only a general, though satisfactory indication can be given. As a transition to direct experience, we have first to point to a remarkable result of the experiments of Bischoff and Voit,[a] who proved that in alteration of diet the amount of water in the body undergoes a modification. When a dog, after having for some time been fed with vegetable matters, got a liberal supply of meat, the water was removed from his body in streams: by the urine alone, 120 grammes (about four ounces) more were excreted than was taken in with all the food and drink together. In accordance herewith the muscles after the use of animal food were richer in solid constituents than after feeding with vegetable matters. Hence it follows, in general, that the accommodation of the animal to a certain food depends on an essential change in its body, and we attach especial value to the particular fact that animal food makes the muscles firmer, richer in solid substance, poorer in water. It might be alleged against the force of these experiments that they were performed upon carnivorous animals—the dog and the cat—that these digested vegetable food less perfectly, and that the increased amount of water in their muscles, when upon vegetable food, is to be looked upon as morbid. But this objection, the value of which we shall not here more closely investigate, in no case affects the experiments of J. Ranke,[b] which led in man to a similar result. These experiments are, in fact, extremely important. It appeared that man, in proportion to his bodily weight, cannot digest nearly so much meat as the dog. This quantity was, moreover, insufficient to support the body. Under the most liberal use possible, during which digestive disturbances were not wholly absent, more carbonic acid was removed by the perspiration than the carbon of the meat consumed could yield. Consequently, some of the fat of the body must have been used, and at the same time the deficiency of nitrogen in the urine and feces was so great that the experimenter was obliged to infer that albuminous matters were deposited in the body. Under these circumstances much more water was removed than was taken in. The change effected by the increased supply of albuminous matters is consequently this, that the body becomes richer in such matters, and at the same time poorer in fat and in water.

In this, too, we recognise a certain accommodation. By the excessive use of meat the human organism evidently approaches more and more to that of the carnivorous animal, and we believe, with Ranke, that in very

[a] Die Gesetze der Ernährung des Fleischfressers. 1860. [The laws of the nutrition of the carnivora.]

[b] Archiv f. Anat., &c., 1862, p 311.

lean, but at the same time healthy men, the metamorphosis of matter agrees more closely with that of carnivorous animals.

X. THE FEEDING OF WORKING ANIMALS.

In order to prove that vegetable food is better adapted for muscular work than animal food is, an appeal has been made to our beasts of burden. These belong, in fact, almost without exception, to the herbivora. But the argument, borrowed from them, has, however, no great value. It is much as if we should infer, from the use of turf and coal for warming, that fats should not be adapted for fuel. Need it be said that we choose the herbivora, in the first place, as beasts of burden, because they are more cheaply kept? In some places people came to the resolution to confine themselves in the zoological garden to herbivorous animals, not in order to make them work, but simply for economy. Other reasons might, undoubtedly, still be adduced why we do not by preference yoke lions and tigers to our carriages.

That carnivorous animals are not otherwise unavailable as beasts of burden is proved by the case of dogs. In the high latitudes of Asia and America, where they are more equally kept than any other animals, they are, as Parry informs us, used for the conveyance of burdens, and they perform, six or eight being yoked to one sledge, day-journeys of from 40 to 50 English miles, with a load of from 800 to 1,000 pounds weight.[a] And with us, too, dogs worked in the country in the treading wheel, in order to churn; in town, under milk and vegetable carts; and occasionally on the high roads, under dog-cars, which, heavily laden, were drawn along, driver and all, sometimes by a couple of dogs, with incredible rapidity. It is known that these dogs work best when, with bread and milk, they are fed on the refuse of meat and bones.

But if, however, herbivorous animals are more advantageous as beasts of burden, the largest in general are to be preferred. Among these are our horses, oxen, asses, and mules; also buffaloes, camels, and dromedaries. Large animals are more suitable for most work, not only because a single one can move a large load, but they are also more advantageous, because, with a smaller surface in proportion to their mass, they lose comparatively less heat, and the consumption of matter can consequently be proportionally more useful in the production of mechanical work.[b] In this point of view the elephant would be the best beast of

[a] These and other particulars I have taken from an address by Bruecke: Die Arbeitsthiere. Wien, 1854. Delivered on the 30th May of that year, at the commemorative sitting of the Imperial Academy.

[b] Conf. De stofwisseling als bron der warmte van planten en dieren. [The metamorphosis of matter as the source of heat in plants and animals.] Utrecht, 1845; as well as Bergmann. Thierische Wärme [Animal Heat] in Wagner's Handwörterbuch.

burden; but for most work he presents practical difficulties, and since the introduction of firearms he has been disused in war.

Now, is it a matter of indifference what food these animals use? Experience has taught the contrary. Highly-bred horses need highly-nitrogenous food. Their excellent qualities are developed under the influence of such aliments, combined with exercise and care for perfect transpiration. Thus, in these horses the cubic centimetre of muscle has reached a higher labour value than in any other working animal. In order not to degenerate they now require such food and require it permanently. The Arab never lets his horse eat grass and straw to satiety. His chief food is barley, and in the wilderness he gets milk, and if great effort be required, even camel's flesh. The horses, which in the Sahara, are used for hunting ostriches, live nearly exclusively on camel's milk and dried beans. Of our horses, too, it is well known that in order to do heavy work they require more than grass and hay. Oats are necessary to give strength and activity; and while running the food preferred for the horses is bread. I have consulted many innkeepers and coachmen upon the feeding of horses. Their verdict has been unanimously as follows:—"The oats must be in them. If they come from the farmer they are round and plump: the farmers feed well. But such horses are not fit for our use. They sweat directly. They cannot bear a long run. If they have had hay the whole time, although they then get plenty of bread and oats for a couple of days, they cannot run upon it. The oats must be in them; they then need not eat much by the way. To feed well constantly, that is the point. Hard running depends upon the breathing. If they have stopped for a time, they must again accustom themselves to running; but in a few days they are ready, provided they have all through had plenty of oats. Grass is not much, but it is good for the health." In these simple words of the man whose eagerness for gain had sharpened his faculties of observation, lies a nucleus of truth. In our language it signifies that horses, in order to perform hard work, must have not watery, but firm muscles. And the food which serves best to produce such muscles is then further necessary to keep them in this condition. It is remarkable that reference is so generally made to the tendency to sweating in a horse not very well fed. Perhaps this is connected with the greater amount of water in the thick rotund body. But certainly in perfect muscles the proportion of work produced to heat is also more favourable. In this, too, exercise bears its part. If albuminous matters yield dry muscles, exercise makes them red. My friend Dr. Luijten[a] showed, many years ago, that the muscles have more colour the more they are used, and that their redness depends principally upon the blood contained in the vessels. One of the results of exercise

[a] De musculorum rubore. Diss. inaug. Trajecti ad Rhenum, 1840.

will therefore be a better circulation in the muscles, on which not only the supply of oxygen and fuel (*sit venia verbo*), but also the removal of the products of the metamorphosis of matter depends. Strikingly correct, too, is the expression, "hard running depends upon the breathing." If the Arabian horses are the most valuable, among the English race we find the fleetest racers. They move with the rapidity of a violent hurricane (25 metres per second), and for a short time leave even the express trains of England behind them. To this, in the first place, a peculiar structure of the limbs contribute; but it depends chiefly upon the lungs, which have in a comparatively short time to exchange the oxygen for the carbonic acid formed. We feel this in ourselves. In a rapid movement, especially in rapidly ascending a hill, our breath in a few minutes becomes too short, before any fatigue worth speaking of is felt in the limbs. Horses which are accustomed daily to do a certain work, step by step, are not in a state to do the same work trotting in fewer hours of the day.

The conclusion is this, that the best and noblest races are developed and maintained upon a diet abounding in albumen, and that each horse is better adapted for work when fed upon oats, bread, and beans, than upon hay and grass.

The labour of oxen is far behind that of horses; these animals are, however, still used here and there, in the Netherlands, too, among others, in the establishments of Veenhuizen and the Ommerschans. In general they live on grass and hay; but if they have hard work to perform they get, with a more liberal allowance of their ordinary food, so-called oxen-bread, baked principally of bean-meal, and conseqently rich in albuminous matters. Without this bread they fail in this work.

What we read of the reindeer is important. Brooke tells us that in one day it not unfrequently runs for nineteen hours, performing 150 English miles, yoked to a sledge of 240 pounds. This work is equal to that of four Esquimaux dogs. Now in the Imperial Menagerie at Schönbrunn a reindeer gets as his daily food 2·24 kilogrammes (nearly five pounds avoirdupois) of Iceland moss. But Brücke tells us that the food of the rein-deer is not always so sparing, that in Summer he seeks the mountain pastures, and in Winter lives on the parasitic plants of the forests, and for want of other nitrogenous food catches and devours field mice.

Moreover, the accounts respecting the long journeys which camels take, laden with heavy burdens, border on the incredible. And yet it is a question whether in these journeys they perform very much work. In estimating this the chief element is, how much the animals, in moving, raise their bodies, and with them the load which they carry. I am not aware that any observations have been made on this point, but I venture to suggest that camels are indebted for their extraordinary

capability of carrying loads partly to the slight displacement undergone by the latter in a vertical direction. Of camels we know that in their large anterior stomach they can take in at once an abundant supply of food and water, and that, moreover, they have a store in their humps, the fat of which, in the absence of food, is consumed on long journeys. In other respects I do not find sufficient information on record, as to the food they get, to draw further inferences, even if we knew what work on a level the movements of their laden body represents.

On the whole, we come to the conclusion that animals, to perform work, *constantly* use not only a larger quantity of food, and therefore proportionately more albuminous matters, but that they further require for their labour a more highly albuminous diet. In this way the body attains a definite stationary condition, and that food is thus consumed and used. Consequently in the excreted matters the same quantity of nitrogen must occur as in the ingesta. The nullity of the argument derived from the slightness of the increase of the urea excreted during the performance of work is thus again made apparent.

XI. THE FOOD OF MAN.

We have approached the great question:—What food does man require? We may put this question generally. It is then not simply with what food is man best adapted for work, but rather, under what relation of the alimentary principles is he capable of the greatest perfection in mind and body. If science can here pronounce no definite opinion, neither has experience solved the problem. Where we, whether in the same people or in people of like race, meet with a great difference, in general not only is difference of food connected therewith, but contrasts exist in many circumstances of a material and moral nature, and our knowledge fails to isolate and estimate the influence of each circumstance. We are, however, not without a leading principle. One fact is established, to which we attach great importance, that man everywhere exhibits a tendency to the use of mixed food, and under its use is well developed and in good health. This fact is, in truth, of much significance. It speaks to us of our ancestry. It teaches us under what circumstances man has become what he is. With the food which he used, his system must have been in harmony, and his inclinations, too, are connected therewith. Many years ago I had shown that the harmony of animal life in general finds its explanation in three laws—that of habit, of exercise, and of hereditariness. The first announces that each animal being is so modified in its organization by the influence to which it is persistently exposed that it harmonises with these influences. The second is, that each organ, each part of the body is so modified under the constant influence of the will or of other circumstances which determine its action, that it corresponds to

what this will or these circumstances require of it. And finally, the law of hereditariness is thus expressed: that the peculiarities of ancestry are transmitted to posterity; this law, therefore, confirms for the subsequent generations what habit and practice have effected in the preceding ones. By these laws, which we find everywhere confirmed by the phenomena,[a] all that we call suitable in nature is recognised as necessary; harmony is only a manifestation of these laws. And if we further reflect that defective objects often die prematurely, without propagating their race, and that also in the remaining ones the conditions for propagation are in general less favourable, we find in the laws described at the same time the foundation of progressive perfection in creation. But is retrogression, then, inconceivable? By no means. If the conditions whereby the nature of organic beings is determined, are no longer to be realized, the race is threatened with retrogression, which may terminate in destruction. So man is also, under the influences of over-population and accumulation in great centres, liable to retrogression. Want of the aliments, under the use of which man has become what he is, may be a very essential factor therein.

On another occasion[b] I have proposed to myself the question:—What, after a series of generations, would become of man if he gradually accustomed himself to an exclusively vegetable or animal diet? I acknowledged that the results thereof could not be foreseen, but at the same time I showed that it would be a rash struggle of the human will against the course of nature to strive after another harmony, whereby, even if it could be effected, its best qualities and noblest gifts might easily be imperilled. Now from time to time some, impelled by certain philosophical or religious prejudices, have advocated the exclusive use of vegetable food. Perhaps such a doctrine should be opposed with some earnestness if we were not convinced that it will never meet with much countenance. Nature is stronger than theory. To the followers of Pythagoras, who rejected the use of animal food, my friend the late Professor Karsten thought that the "naturam expelles furcâ, tamen usque recurret" might also be safely applied. Meanwhile, wonders are related of the sect of the *Vegetarians*. The children of their first apostle, a certain John Newton, who in 1811 made his appearance with his doctrine, were said to excel in health and beauty, and to be perfect models for a sculptor. If the statement is true, is it nevertheless not absurd to connect the beauty of the bodily forms with the diet followed in a single generation? Let natural science beware of extending the hand to anomalies of the religious sentiment.

[a] More fully developed in my inaugural address: De harmonie van het dierlijk leven de openbaring van wetten. [The harmony of animal life a manifestation of laws.] Utrecht, 1848.

[b] De voedingsbeginselen. [The principles of nutrition.] 1858, p. 40.

We may now further inquire:—What does daily experience teach respecting the more direct influence of diet? We have thus far spoken of vegetable and animal food. Evidently, however, the question refers only to the proportion of the alimentary principles. In general we may say that fats, and especially albuminous matters, are more fully represented in animal than in vegetable food. But fats are fats, and they will not differ in their action on the animal system, whether they be of vegetable or of animal origin. The same is true of albuminous matters. Therefore, too, if there be any difference, for example, between the use of bread and butter and of meat and potatoes, this will be found rather in the accompanying alimentary principles than in the essential necessary elements—albuminous matters, fats and carbo-hydrates—it being understood that these, in both cases, are present in equal proportion.

Now, experience teaches that a vegetable diet, with excess of carbo-hydrates, promotes the deposition of fat in the body. The animals we fatten are all herbivorous; and for a particular species, too, such as the pig, which belongs to the omnivora, potatoes are quite enough. Even dogs and cats become stout and fat on vegetable food—a proof that it is not so much the kind of animal as the nature of the food which makes the difference; and, just as any other carnivorous animal, they always become, on the most liberal supply of meat (of course without much fat) comparatively lean. In accordance herewith we see excessive deposition of fat in man, too, disappear through the use of animal diet. William Banting,[a] like many before and after him, has experienced the truth of this in his own person; and the experiments of Ranke, communicated above (p. 488) supply the key to it. Now, if it be true that in the animal system fatty matters are formed not from carbo-hydrates, but precisely from albuminous matters, as becomes more and more probable, it is sufficiently evident that the formation and deposition of fat are connected with very different conditions.

From our remarks, as well as from the facts adduced, we arrive at the conclusion that all men ought not to be fed in the same manner. Their diet differs in different nations, partly in connexion with the climate they inhabit, partly in connexion with what the country yields, and by mutual interchange both parties would lose. In the same people, too, the feeding must vary according to birth, position, calling, and habit. Even the requirements of the individual must be attended to. With a tendency to excessive deposition of fat, which interferes with

[a] This William Banting, a well known and esteemed inhabitant of London (formerly "a furnisher" of the "fashionable world"), in a "Letter on Corpulence, addressed to the Public," London, 1863, describes with as much simplicity as unmistakable good faith, the favourable influence which animal diet speedily had on the excessive deposition of fat which had for years oppressed him.

many functions, albuminous matters should be freely given, while the use of fats and carbo-hydrates should be limited, and the latter, provided they are well borne, may be advantageous to the lean. An intelligent man will in this respect consult his own experience.

In particular we have finally to ask:—On what diet is man best adapted for muscular work? In connexion with this inquiry I cannot suppress the remark, that in my opinion a too absolute value has been attached to capability of muscular work. It was as if man was to live exclusively for his muscles, and as if, with the acquirement of the greatest muscular power, the most perfect health and the highest development in every direction would also be attained. This partiality has also sought advantages in gymnastic exercises, which I cannot possibly find in them, unless, perhaps, as corrective of the injuries proceeding from constrained sedentariness in schools, against which the natural movements of running, leaping, and wrestling may do quite as much, without giving rise to disproportionate and consequently unpractical muscular development. We cannot overlook that with the increase of civilization the muscular work required of the people gradually diminishes Everywhere the tendency is to replace this by machine work, for the very valid reason, that mechanical work is under no form so costly as under that of muscular work, especially of men. It is true that we obtain, in reference to the fuel (food) consumed, more useful work than in the best steam-engine;[a] but the fuel is amazingly dear in comparison with the price of turf and coal. Thus we should be able to justify the proposition that the worst use to make of a man is to employ him exclusively in mechanical work.

But the question:—On what food man is best suited for mechanical work has in this way by no means lost its importance. Muscular labour is still connected with all work, and in some kinds of work powerful exertion of the muscles is always involved—digging, ploughing, and thrashing, the hoisting or carrying up of heavy loads, the handling of heavy hammers or hatchets, rowing, sawing, ramming, &c.; also long marches, with bag and baggage, and various exercises in the management of arms, are to be enumerated. Moreover, muscular work, in a certain measure, will always be required in the interest of the organism. That albuminous matters are in the first place necessary for this purpose, and that they should be more liberally represented in the food of the working man than is the case with us, was some years ago argued by Mulder with an enthusiasm which was the result of a deeply-felt conviction. This conviction communicated itself to all. On no side was it opposed. It was not asked whether muscular work was produced directly from the albuminous matters—the law of the maintenance of energy had as

[a] Conf. Helmholtz, Lectures. *L. c.* Lecture vi.

yet found but little application: but he reasoned simply thus: the muscles must perform work, and they must therefore, in the first instance, be well nourished, and indeed with albuminous matters, of which they are chiefly composed. Now, after it was on the one hand proved that non-nitrogenous matters were also capable of producing mechanical work, and after it was on the other hand shown that in this work a proportionate metamorphosis of albuminous matters does not always take place, some began to abandon their conviction. And soon they now came forward with the assertion that heavy work is performed with a slight supply of albumen, and that it is precisely the consumers of albumen who in general use their muscles but little. The last statement is perfectly true; it proves, however, as we have already said, only that also without muscular work much albumen may be metamorphosed. The first we deny. Science has led us to infer that a supply of albuminous matters makes the muscles better adapted for work, and daily experience fully confirms this. The argument made use of we reject for the following reasons:—

1. People are deceived as to the amount of work performed. Most callings require rather an accurate use than great tension of the muscles. In fact the work performed by the human hand is, when measured by the accurate standard of kilogrammeters, in general very little. If the manufacturer of fabrics was called to perform hard work, to carry heavy loads, or to overcome great resistance, it would soon be seen how little he is fitted for the purpose. Even his very appearance betrays it. On slender food and under unfavourable circumstances his physical condition is lowered to a degree at which it can now be maintained by that slender diet. He gets little, especially of albuminous matter, and has scarcely need of more (compare p. 487). At the same time he has lost the bodily form, the shape, the weight even of the normal man. For him special statistics exist. Thus he has the true capability for working in his manufacture. Involuntarily he appears before our mind's eye when we read in Bruecke:—"Scantily nourished on thistles and weeds, the ass has become the slave of the poor man, who feeds his beast by the wayside: he is the *proletary* among the beasts of burden, in whose degraded form we no longer recognise the strong and beautiful animal who formerly, decorated with costly trappings, bore on his back the princes of the East;" and the "quantum mutatus ab illo" is on our lips as we think of the old Batavians.

2°. They who actually perform work also use albumen, and in general consume more in proportion to the severity of the labour *a.* The quantity of food is great, surprisingly great, we will say, if we visit the tables of the country people, who do heavy work. Proportionate thereto is the albumen ingested. *b.* They get more albuminous matters than one suspects. Besides potatoes they use different kinds of flour and

bread in great quantity, and every evening trencherfuls of buttermilk, which contains all the casein of the milk; and in the middle of the day pork and bacon. For soldiers in garrison Mulder calculated 100 grammes of albuminous matters daily; sailors receive still more. *c.* In a yard visited by me some men were employed the greater part of the day sawing planks. The strongest were chosen for it. They got higher wages and required it, for "without a pound of meat daily and a good jug of beer one could never hold out at that work." Thus, too, the guide, who daily ascends mountains, helps the traveller and carries his valise, must be well fed and especially requires animal food. Of the English labourer, who daily gets meat and drinks strong beer, we know that he is strong and active, and the Irish people, who live almost exclusively on potatoes, are called lazy and slothful. I have often remarked that slothfulness is rather a morbid symptom than a vice.

The conclusion is this:—Muscular work and heat arise in the animal organism, both being derived from the chemical energy as well of non-nitrogenous as of nitrogenous matters. Of both kinds of food the animal system has need. In the body there exists a certain relation between heat produced and muscular work. By exercise this relation becomes more favourable for muscular work. A liberal supply of albuminous matters tells favourably in the same. The reason of this is probably to be found in the better nourished and firmer condition of the muscles and of the whole body, which is obtained by means of a more highly albuminous diet. The development of man in general appears to attain the highest pitch under the use of a mixed diet.

CLINICAL RECORDS.

LONDONDERRY CITY AND COUNTY INFIRMARY, AND CITY FEVER HOSPITAL.

REPORT FOR 1865.

By T. H. Babington, M.D., T.C.D., M.R.I.A., Surgeon to the Infirmary.

Return of Patients admitted into and Treated in the City and County of Londonderry Infirmary, during the year ending 31st December, 1865.

Remaining 31st December, 1864,	53	
Admitted,	571	—Total, 624
Discharged, cured and relieved,	511	
Irregular,	2	
Left at own desire,	31	
Incurable,	6	
Died,	35	—Total, 585
Remaining 31st December, 1865,	39	

City of Londonderry Fever Hospital.

Remaining 31st December, 1864,	26	
Admitted,	268	Total, 294
Discharged cured,	257	
Died,	20	Total, 277
Remaining 31st December, 1865,	17	

Number of Beds in Hospital,	48	Male.
Ditto,	24	Female.
Total,	72	
Number of Beds in Fever Hospital,	32	
Total number of Days passed by Patients in Hospital,	14,777	
Total number of Days passed by Patients in Fever Hospital,	7,057	

Average Cost of Hospital and Fever Patients, including all expenses,	£1	15'	5¼
Average Cost of Hospital and Fever Patients, exclusive of Salaries, and Wages, amounting to £506 2s. 9d.,	1	4	5
Daily Cost of each Patient, including all expenses,	0	1	6
Daily Cost of each Patient, exclusive of Salaries and Wages, amounting to £506 2s. 9d.,	0	1	0¼

Numerical Abstract of Cases of Accidents and Diseases admitted in year 1865.

ACCIDENTS.	No.
Burns and Scalds,	6
Bite of Dog,	2
Dislocation of Hip,	1
,, Shoulder,	3
,, Tibia,	1
Fracture of Acromion,	1
,, Forearm,	8
,, Humerus,	3
,, Clavicle,	1
,, Lower Jaw,	1
Fracture of Leg,	6
,, Ribs,	2
,, Thigh,	6
Compound Fracture of Arm,	1
,, ,, Leg,	1
Injuries of Head,	19
Rupture of Stomach,	1
Ununited Fracture of Humerus,	1
Wounds and Contusions,	86
Wound of Throat, suicidal,	1

DISEASES.	Cases.
Abscess,	10
Ague,	2
Anemia,	3
Aneurism of Aorta,	1
,, Popliteal Artery,	1
Anthrax,	3
Cancer of Breast,	1
,, Face,	3
,, Lip,	7
,, Stomach,	2
Diabetes,	2
Diseases of Brain, &c.,	21
,, Bones and Joints,	18
,, Eyes,	23
,, Heart,	4
,, Lungs, &c.,	67[a]
,, Stomach, Liver, &c.,	53
,, Skin,	34
Diseases of Testicle,	4
,, Uterus,	11
Erysipelas,	7
Excessive Intoxication,	2
Fistula in Ano,	1
Hernia,	6
Hare-lip,	2
Hydrocele,	4
Ovarian Disease,	1
Paronychia,	10
Polypus of Nose,	1
Retention of Urine,	8
Rheumatism,	25
Struma,	6
Syphilis,	19
Tumours,	6
Ulcers,	33
Fever,	268

[a] Including bronchitis, 34; phthisis, 25; pleuritis, 5; pneumonia, 3.

Diseases and Accidents which proved Fatal.

	Died.		Died.
Diseases of Brain,	2	Heart Disease,	1
Albuminuria,	3	Injuries of Head,	3
Aneurism of Aorta,	1	Phthisis,	6
Bronchitis,	4	Pneumonia,	2
Dropsy,	2	Periostitis, &c.,	1
Dysentery,	1	Ovarian Disease,	1
General Debility,	1	Rupture of Stomach,	1
Hernia,	1	Urinary Fever,	1
Hepatitis,	1	Tabes Mesenterica,	1
Hectic Fever,	2	Fever,	20

Operations.

	No.
Amputations of Thigh,	2, one died.
Amputation of Breast,	1, for cancer.
Amputation of Thumb,	1, for exostoris.
Excision of Half of Lower Jaw,	1.
Operation for Strangulated Hernia,	1, died four hours after.
Operation for Contraction of Fingers from burn,	1, cured.
Removal of Cancerous Lips,	7.
Operations for Hare-lip,	2.

Some of the cases in foregoing abstract are deserving of a more extended notice. It will not be inconvenient to remark on each in its numerical order :—

1. *Dislocation of the Tibia.*—This accident occurred to an elderly man falling over the curb-stone of the flagging, as he came out of a hall-door at night. The fibula was fractured, and the tibia dislocated forwards on the astragalus. He came under treatment a fortnight after the accident. The dislocation was reduced with some difficulty, the limb firmly bandaged with a splint behind the heel, and all placed on a M'Intire's splint. The patient made a good recovery, and is walking about with but a slight halt.

2. *Injuries of the Head*, at all times formidable, and even the most trifling, demand serious attention and utmost vigilance. We had three fatal cases during the year :—The first, a passenger on board an emigrant ship, sailed for New York on 24th May, 1865. The ship came to anchor about sixteen miles down the river, and about ten o'clock, p.m., the patient was found lying in the hold, where the passengers' luggage had been stowed, totally insensible, having fallen down the hatchway. He was brought back to Londonderry in a steamer, and received into hospital at four o'clock, a.m., on the 25th. He was totally insensible, very cold, and almost pulseless—in fact, I thought he was dying. Heat was applied to his extremities and the whole surface of his body, and some warm wine and water administered in such quantities as could be got down his throat. In a few hours he was somewhat restored, his

pulse improved, but he still remained speechless and insensible. The frontal bone, and a portion of the parietal on right side, were fractured and depressed. A crucial incision was made over these, the flaps dissected up, the fractured and depressed pieces of bone removed; there was no blood extravasated on the dura mater. The patient immediately spoke, told his name, where he lived, and the name of his clergyman. He continued sensible, and we had good hopes of his recovery; but on the third day he became stupid and insensible, passed his urine and feces under him, and died on the evening of the 28th May. On examination there was a considerable quantity of pus under the dura mater, and the fractures had extended down through the temporal bone and all along the parietal, but all the depressed portions had been removed.

The second fatal case was the result of concussion. A horse ran away with a cart, which was overturned, and the patient tossed out, his head striking against a wall. He never rallied, remained totally insensible, and died in thirty-six hours. There was no fracture, nor any extravasation of blood.

The third case (*fracture*).—A sailor fell from the mast of his vessel, and sustained a compound fracture of right humerus, and severe injury of head. On admission four hours after the accident he was totally insensible, bleeding from right ear—and, in fact, dying. He expired three hours after his admission. The frontal, temporal, and parietal bones on right side were all fractured, and blood extravasated under the cranium, on the surface of the dura mater.

Rupture of Stomach.—A stout healthy young man, attending masons working at new gas works in course of erection, fell from a scaffold near the roof, a height of about forty-five feet. How he fell no one could tell; it was supposed his head struck against some piece of timber in his fall, as there was a severe contusion on his right temple; he was found on the ground in a doubled-up posture, his right side lying against some blocks of stone. He was brought to hospital at once, about one o'clock, p.m. He complained of intense agony all over his bowels; he was vomiting, nearly pulseless, his skin quite pale, and icy cold; no pain in his head, nor any confusion of intellect—in fact, his symptoms were intense collapse, and violent pain all over the stomach and abdomen, and he exclaimed:—"There is something burning there," pointing to his stomach—"take it out of that." He expired about two hours after his admission. On examination there was found a rent in his stomach, on the anterior surface, about two inches long, a complete tear, situated about three inches from the pylorus, and through this opening the contents of the stomach, which looked like stirabout, had escaped. The stomach was perfectly sound in every other respect.

Ununited Fracture of Humerus, cured by seton.—A stout muscular man, a carter, admitted into infirmary on 26th December, 1865. He had got

his humerus fractured four months before; he said he could make no use of it. On examination it was evident that union had not taken place; the ends of the bones were not in apposition, they lay sideways by each other, the space between them filled with a substance feeling like cartilage, but soft, and he was unable to lift his arm. On the 1st January, with the assistance and concurrence of my friends, Dr. White and Dr. Rogan, I passed a seton (a skein of silk) through the arm, between the ends of the bones. An interval of ten days elapsed before much inflammatory action set in, subsequently an abscess formed, and a second one, with a good deal of irritation, and at the end of the third week I withdrew the seton. The arm was kept firmly adjusted in splints, the situation of the seton being kept uncovered. I have had the satisfaction of sending him home to a distant part of the county, on the 20th March, with a firmly united arm. During the treatment he was generously fed, and took fifteen grains super-phosphate of lime, three times a day.

Aneurism of Aorta.—The patient in this case had been long under observation. He became blind before his death, and finally sunk under a long protracted attack of chronic pleuritis.

Aneurism of Popliteal Artery, Cured by Compression.—I had the gratification of publishing, in Vol. XXIV. of *Dublin Quarterly Journal of Medical Science*, "Notes of a Case of Popliteal Aneurism, Cured by Compression;" the treatment of the case extended from 20th August till 29th September. I have now the pleasure of recording another successful case of same disease, cured by compression, but in which the treatment, although in the end successful, was protracted to an unusual extent. I was, however, in the result amply rewarded for patience and perseverance. It would be tedious and unprofitable to detail minutely the daily notes of a case, the treatment of which extended from the 28th September, 1865, when the pressure was first applied, till the 30th January, 1866, the day on which the disease was cured:—William Neely, a sawyer by trade, a pale anemic-looking man, aged thirty-five, had always enjoyed fair health, but had the constitutional defect that the slightest scratch or wound was always peculiarly slow in healing; his blood seemed weak and poor, and deficient in fibrin and red globules. While engaged with others in lifting a log of timber, the log rolled back on them; he made additional exertion to resist it, and felt an uneasiness not amounting to a pain in his left leg, behind the knee. He went to his work the next and following day; and the third day, his limb feeling very stiff, he consulted my friend, Dr. Edward Smith, who detected the existence of an aneurism in the left popliteal space, and recommended him to come into hospital, which he did. He was placed on a hair mattress, ordered good diet, porter, and not much fluid, and an opium pill every sixth hour. On the 28th September, at noon, the circular compressor was placed round his thigh, and commanded the circulation completely; he wore it screwed down for

two and a half hours. It was kept on for four days and nights, but the pressure had to be slackened every two or three hours; it was then removed, the skin having ulcerated. Cartes' saddle was put on, and pressure applied below Pourpart's ligament. He with difficulty bore this pressure for two hours at a time; it had constantly to be slackened, and on the 16th October it had to be removed, the skin in the groin having ulcerated, and the whole limb having become œdematous. Notwithstanding the frequent interruptions to the treatment, the tumour had acquired a feeling of considerable firmness and solidity. He was now left quiet, without any treatment, except firmly flexing the leg and keeping it so; this produced no good result. He complained so much of the uneasiness caused by Carte's compressor, that I procured one of Read's compressors, which I applied on 4th November. He bore this better, but could not bear continued pressure for more than three or four hours; the pressure was kept up at intervals till 21st December, when he left hospital, and returned 5th January. On the 6th January, Read's compressor was again applied, and worn with little interruption for three days and nights. He was also ordered fifteen grains saccharated carbonate of iron three times a day. On the 10th we had to remove the instrument, but there was a marked improvement in the tumour; it was becoming solid, circumscribed, and the pulsation very feeble. The abraded surface having healed, the compressor was again applied, on the 26th January, and screwed down. On the morning of the 30th, on visiting the man, he said :—"Please, sir, examine my leg, for at four o'clock this morning I felt a most unusual sensation in it, as if I was stabbed through the joint; and I called to the patient opposite; there is something very queer in my leg." On minutely examining his leg, all pulsation had ceased. He remained in hospital for ten days afterwards, and is now daily at his work.

In the treatment of this case I was frequently disheartened and discouraged, often hopeless as to the result, and more than once disposed to tie the artery, but deterred by the weak constitution I had to deal with. We have, however, been rewarded for our patience and perseverance. I beg to bear testimony to the efficacy of Read's instrument.

Ovarian Disease; fatal.—The notes of this case, and the morbid specimens, were brought before the Obstetrical Society by my friend, Dr. Denham, at the meeting of 10th March.

Periostitis and Synovitis of Left Humerus.—A case of this formidable disease—several cases of which are graphically described by the masterhand of the late Mr. M'Dowel in the numbers of the *Dublin Journal of Medical Science* for the months of July and September, 1833 :—Thomas M'Laughlin, aged eighteen, admitted into hospital 26th July; complained of pain in his left shoulder; said he had sprained it two weeks since, wheeling a barrow; had rubbed it with turpentine, but it was no better.

On examining it, he complained of excruciating pain, greatly increased by pressure and motion; the parts round the shoulder joint were swollen and tense, and the skin rather red. He, on being questioned, said he had suffered very much pain, and got no sleep; he was very thirsty, and had all the general symptoms of high inflammatory fever, and much constitutional disturbance. I at once said to Dr. Rawson, my house-surgeon, now surgeon to Baltinglass Infirmary and Dispensary—"That is no case of sprain, but a case of that formidable affection of the periosteum and synovial membrane described by late Mr. M'Dowel, and I am afraid will end fatally." A large number of leeches were applied over the joint, and around it; warm fomentations; calomel and opium administered internally. Next day there was no improvement; the leeches were again applied. On the fourth morning, the 29th, there being a feeling, but an indistinct one, of fluctuation, I made two deep incisions over the joint, and along the deltoid muscle; little, very little pus escaped, but the tension was relieved. The following morning, the 30th, I made another incision, and on passing a director into the incision it went direct into the joint; the synovial membrane had given way, and the periosteum was detached from the head of the bone. 30th. He complained this morning, the sixth from his admission, of acute pain in his right side, and his breathing was very rapid; inflammation had set in in the lower lobe of his right lung. He sank rapidly, and died on the morning of 1st of August.

The examination after death revealed appearances as described by Mr. M'Dowel:—The muscles infiltrated with pus, synovial capsule of joint opened, periosteum detached from head, neck, and shaft of humerus, and the head of the bone corroded; the lower lobe of right lung inflamed and filled with numerous, small, distinct, purulent deposits.

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